

Contemporary Psychology

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Is Management Going Human?

Douglas McGregor

The Human Side of Enterprise. New York: McGraw-Hill, 1960. Pp. x + 246. \$4.95.

The author, Dr. McGregor, is Professor of Management at the Massachusetts Institute of Technology. He has from Harvard University a PhD in experimental psychology (of all things!) in 1935. For two years he was instructor in psychology at Harvard, then for eleven years a member of the staff of MIT's Industrial Relations Section and for five years its Director. After that he was President of Antioch College for six years, and then came to his present post. He has always been primarily interested in human relations and has published variously in this sector of industrial psychology. The present book is his first. Two of CP's Consultants (not McGregor!) thought this volume timely and important and urged a double review by contrasting reviewers. Here are the chosen reviewers, contrasting in many ways and mutually redundant in others.

Corporate Management in a Social Vacuum

By DANIEL KATZ

The first reviewer, Dr. Katz, is Professor of Psychology at the University of Michigan. He has a PhD from Syracuse University in 1928 obtained under Floyd Allport. He taught at Princeton for five years and then at Brooklyn College for four years, after which in 1947 he went to Michigan. He has been a research director in the Surveys Division of the Office of War Information, and at Michigan director of the program, Human Relations and Organizational Behavior, conducted by the Michigan Survey Center. He has been concerned with the classical organization theory of Gulick and Taylor and has published variously on the psychological appraisal of the machine theory of organization.

THOUGH aimed at a management audience, this book should prove profitable reading for psychologists. It applies knowledge and theory from relevant areas of psychology to the management of industrial enterprises. The psychologist, already familiar with this field, will be interested in the way in which McGregor presents his case to business leaders. The psychologist, unacquainted with current thinking about organizational functioning, will gain new insights into administrative problems. The novelty lies not in original theory or discovery but in the application of known motivational principles to the everyday working situation. The author has faith in the validity of psychological findings about what energizes and gratifies people and has the courage and originality to believe that the negation of these ideas is not the way to run a business.

The heart of the case is that the conventional theories and practices in industrial organizations are based upon poorly applied notions of reward and punishment and ignore the positive ego motivations of accomplishment, self-expression, and self-development. Current management policies not only fail to utilize the resources of a cooperating, interacting group of people who become committed to group goals, but they lead to these forces' being arraigned in opposition to organizational objectives. The author brings his thesis into sharp focus by contrasting Theory X, the traditional view of direction and control, with Theory Y, the integration of individual and organizational goals. Theory X assumes that people have an inherent dislike of work and must be bludgeoned into it. It further assumes that they seek security, avoid responsibility and essentially want to be directed and ordered about. Theory Y, on the other hand, assumes that people find satisfaction in the exercise of their abilities, that they blossom under responsibility, and that modern industrial life makes but partial use of their intellectual capacities.

The confrontation of the two opposed theories is a restatement of the doctrine of the opponents of the machine theory of organization. The author's contribution lies in working out the implications of this doctrine for such specific problems as performance appraisal, the administration of salaries and promotions, incentive pay, profit sharing, democratic practices of participation, staff-line relationships, and management developmental programs. He points out, for example, that staff-line relationships become complicated by the upper-level manager who delegates some of his responsibility

ties down the line and then utilizes the staff to perform a police function, thus preserving his control. McGregor's solution is to abandon Theory X with its reliance on authority and on surveillance, to recognize other forms of influence than authority, and to encourage genuine delegation with an emphasis upon the self-control of subordinates and superiors alike. Then staff and line can cooperate as a team in attacking common problems.

The author's extensive practical experience, both as an industrial consultant and as a college president, his lack of rigidity in the application of his point of view, and his ability to assess the significant aspects of a situation contribute materially to our understanding of some of the problems confronting large-scale organizations; but what is more valuable is that his implementation of a psychological theory of management opens the way to further fruitful applications. His book is essentially the presentation of a point of view and its acceptance by some sectors of management would lead to desirable changes in industrial organizations.

IN some ways, however, the author has not made the strongest case for his position. The book lacks documentation and is heavily anecdotal in form. This pattern was deliberate on the part of the author, since he was writing for practitioners and not for social scientists. Nevertheless an occasional citing of a research study specifically relevant to the problem under consideration might add conviction. Many of his readers, whether in the social sciences or in management, would love to see the evidence for the success of the Scanlon plan and the conditions under which it works best—for, the Scanlon plan, combining as it does a form of profit sharing with democratic participation, will be dismissed as Utopian unless facts and figures can be cited. Moreover, even if tables and graphs are assumed to be too cumbersome for popular presentation, it would help if attention had been adequately called to available sources of research documentation. The references are loosely added at the ends of chapters, and loosely used, and there are many lapses in failing to refer to the

most significant studies corroborative of the points made. There is no index. This, then, is a highly personal document with the advantages and disadvantages of such a personalized form of communication.

On the substantive side the glaring weakness of the book is the individualistic emphasis upon motivational principles with no theory about the functioning of the social system to which they are to be applied. There is little explicit realization of the social structural factors which influence the adoption of different management philosophies and



DOUGLAS MCGREGOR

strategies. It is no accident that the Scanlon Plan has been adopted in small or marginal plants, nor is it fortuitous that General Motors follows machine theory in its production of automobiles. Another example is found in the way in which the technological requirements of automation with the heavy investment in electronic programming call for management skills of a new order—skills in assessing the trends in the total political, economic, and social pattern and in cooperative problem-solving by a cabinet of leaders. Some of McGregor's ideas are already operative in the top sectors of such automated companies. In other words, the author does not recognize the structures and substructures which have to change in order for his principles to work. Granted the desirability of utilizing the human resources in an organization more fully by integrating

the goals of the individual with the objectives of the organization, how is this result brought about? Organizational change means more than persuading some receptive member of management about the need for a particular reform. The desired change has to be related to some critical point where the organization as an open social system is dependent upon environmental feedback. And some new structure has to be added which gears into the major subsystems of the organization, or the change will be neither real nor lasting.

THis neglect of any theory of organizational structure and functioning is responsible for the inability of the author to come to grips with problems of authority and power in a social system. He contends that there are modes of influence other than authority and that these non-authority modes are coming to be exercised more commonly. His observation on this score is sound and the extension of democratic values into various types of organizations is a general trend in western countries in this century. Nevertheless, the legitimate basis of authority in an organization is a dominant factor that influences the nature of its democratic and participative practices. McGregor, however, regards participation as a range of managerial actions from minor forms of consultation to full-scale decision making and states "there is no implication that more participation is better than less" (p. 126). Nevertheless there is a critical difference between participation in decision making where the group has the power to make its decisions stick, and participation where the group has little or no power to affect the ultimate decision.

This difference is qualitative not quantitative and again has to do with organizational structure and character. Organizations may be authoritarian and hierarchical in the source and distribution of power or they may be democratic in structure with the power to elect representatives and to vote on major policies vested in the membership. To talk about participation, decision-making, integration, and managerial climate without a consideration of the power structure of the system is to talk a very

limited type of language. The integration theory of the author will always run up against the difficulty of tying the rank-and-file members into the organization because of the undemocratic power structure of the industrial enterprise. This is why it does not make too much difference to workers whether their bosses attempt to follow Theory X or Theory Y. They believe in Theory A, namely that the organization which represents their interests and in which they have a controlling voice, namely their own union, has a major claim on their cooperation and commitment.

This general disregard of organizational structure as it affects the great bulk of employees is also in evidence in the author's concentration upon the salaried officers of the enterprise. With the exception of pay incentives and the Scanlon plan, he makes but small attempt to translate the theory of integration for the little fellow. And yet the little people in organizations are those whose jobs are most fractionated, most routinized and most unrewarding, and who suffer the most from the evils of job alienation. A book about the human side of enterprise might give attention to the enlisted men as well as to the commissioned officers. The men in the foundry or the women on the clerical assembly line may be little more than robots in large-scale enterprise but they still perceive themselves as human beings.

In brief, this book presents to industrial leaders two opposed philosophies of management with respect to the motivation of men. The application of these alternative points of view to management's own problems should make it incumbent upon the thoughtful business administrator to re-examine his own basic assumptions about human nature. He will, however, find little help in this volume for his understanding of how to apply motivational principles within an organizational context, for the book gives but scant attention to the nature of organizational structure and functioning.

Corporate Management: Why X Yields to Y

By CARROLL L. SHARTLE

The second reviewer, Dr. Shartle, is Professor of Psychology at The Ohio State University and Chairman of its Personnel Research Board. Before getting a PhD from Ohio State in 1933 he got started in industrial psychology with the Milwaukee Electric Railway and Light Company. He was Chief of the Division of Occupational Analysis and Manning Tables of the War Manpower Commission during World War II. He was Director of Research in the U. S. Air Force's Human Resources Research Institute during the Korean War. He has been Director of the Ohio State Leadership Studies, of which twelve monographs have appeared. He is the author of Executive Performance and Leadership (Prentice Hall, 1956; CP, Mar. 1957, 2, 60).

THIS book contrasts two kinds of management, X and Y. The author calls them theories and supports the broad thesis that the "theoretical assumptions management holds about controlling its human resources determine the whole character of the enterprise." The author identifies himself as a social scientist and is presumably basing his writing on the findings from research over the past years.

Theory X, he claims, is "the traditional view," the view to be found in most of the literature on organization, where it is assumed that (1) human beings have an inherent dislike for work and that (2) most people must be coerced, controlled, directed, and threatened with punishment to get them to produce adequately. This, as presented, is pretty much a straw man. First of all, the proponents are not identified. It is customary in social science to identify and give full credit to writers that hold an opposing theoretical view. Since the author seems to think that X is so wide spread, he should have no trouble listing several leading, typical, modern works on management, possibly one or two from MIT. There is a list of references at the end of the chapter on X. The reader might well infer that these

authors, including Alvin Gouldner and William F. Whyte, exemplify X.

Later in the book the author himself seems to cast doubt upon X. He tells of management's adopting generally far more humanitarian sets of values and adds that the use of authority is still essential and that punishment is sometimes necessary. It begins to appear that X may be the kind of management advocated possibly twenty, thirty, or more years ago.

Theory Y, the good philosophy, is based on the assumptions that man does not inherently dislike work; external control and threat of punishment are not the only means for inducing effort; the average human being learns not only to accept but to seek responsibility; the capacity to exercise a high degree of imagination is widely distributed in the population; and the intellectual potential of the average human being is only partially utilized.

While the book is largely concerned with an explanation of Y and its advantages in application, the author curiously enough frequently adopts theory X in his writing. The general absence of documentation throughout the book—a serious defect—denies credit to those entitled to it and gives an authoritarian posture to the presentation. Furthermore, the unidentified individuals who disagree are brushed aside too quickly, and some receive pretty harsh treatment even to the point of being called ignorant.

The application of theory Y is given in several chapters with numerous examples but rarely with scientific evidence. The author is critical of the use of measuring techniques because the validity of some is not known; yet he fails to provide even elementary scientific validity for the evaluative procedures that he proposes. He indicates that personality and clinical diagnosis for administrative purposes are inherent tools of manipulation and exploitation, and that this danger will be intensified as validity increases. In general, he seems to fear anything quantitative, because it might be used against someone. Here the author is consistent, for practically no quantitative information appears in the book.

McGregor states that Y can take many forms. He places considerable emphasis on the Scanlon Plan. The plan of cost sharing and participation for industry has been promoted at MIT for a number of years. There are fourteen pages of favorable explanation of the plan and four pages in which some questions are raised about limitations. The number of establishments that have abandoned the plan (if any), or who found it sufficiently useful to complete it, is not given. We do not know how many companies are using it, or how these establishments have fared in comparison with other similar establishments who are using other plans, conventional or unconventional. Have there been any evaluations by disinterested investigators whose methods meet scientific standards?

McGregor presents a quite persuasive argument for greater participation in management, a topic often discussed in management circles. A chapter on managerial climate and two chapters on line and staff relationships make good sense and are in keeping with newer developments. Management development is advocated as "growing" talent rather than manufacturing it.

One assumption of theory Y is that unless individuals and organizational goals are integrated, the organization will suffer. The criteria for "suffering" are not given; however, an equally good case can be made that this ideal integration may mean organizational stagnation. A business firm that is on the move in our competitive, changing society is probably not in such a state. From what we know in the psychology of individual differences, and what is known of changing environmental and job demands, imbalances are normal and may be good symptoms not only for the organization but for individual freedom as well.

INITIAL selection of persons for an organization is hardly mentioned. With 1,500 different industries in the United States and about four million different firms, and with considerable mobility in the labor force, it would seem that much could be achieved on the human side of enterprise by careful selection of persons whose needs, interests, and abilities

are likely to find congenial expression in the objectives of the particular company.

Position descriptions for management personnel are frowned upon because they serve as limitations. The author does not seem to realize that duties can be formulated in writing, as well as orally, that the specifications can be open-ended and can give latitude and stimulus for growth. His shying away from written material in general seems to make him favor an undue reliance on the memory of managerial people. The arguments he presents are often weak from a scientific point of view because he presents a poorly done example of a procedure he disapproves of and compares it with a well-done example of a method he advocates.

In discussing the acquisition of managerial skills, McGregor reports two educational methods in current use that appear to bring significant improvement in social interaction. One is psychotherapy, and the other is group dynamics. Again, he cites no empirical evidence. Group dynamics gets almost as lengthy a play as the Scanlon Plan; however, it is more intense and more polemical. Persons who criticize these programs in terms of what group dynamics methods may do to undermine individualism or stifle creativity are given a sample of the theory-X treatment. The author says that these critics are basing their evaluations on "complete ignorance" of the methods used. Also, in discussing W. H. Whyte's thesis that we give undue emphasis to group phenomena, he says that the critics are suffering from ignorance based on underemphasis, not overemphasis, of group behavior. This statement comes close to saying that no one is qualified to evaluate something unless he is already a believer in it.

In a concluding three-page statement following the chapters, the author almost reverses himself. He becomes conciliatory; in fact, he drops the ball. The purpose of the book, he says, had not been to entice management to choose between X and Y but merely to encourage the realization that theory is important. This purpose has a very good chance of being achieved.

An Unusual Text on Statistical Method

Solomon Diamond

Information and Error: An Introduction to Statistical Analysis.
New York: Basic Books, 1959.
Pp. xii + 307. \$5.00.

Reviewed by FRED D. SHEFFIELD

The author, Dr. Diamond, who is Professor of Psychology at Los Angeles State College, escaped having a knowledge of statistical method forced upon him in the process of getting his PhD, but learned, one might say, directly from R. A. Fisher himself by reading Statistical Methods for Research Workers because he needed the knowledge for his research. Now he writes the sort of book he wishes he had had. He is the author of Personality and Temperament (Harper, 1957; CP, Dec. 1958, 3, 355f.). The reviewer, Dr. Sheffield, is Associate Professor of Psychology at Yale University, where he has been teaching inferential statistics to graduate students for thirteen years. He is a Yale PhD but he studied mathematics at the University of Washington before that. Primarily he is a learning psychologist—a contiguist—who is now shifting his focus to perception and symbolic behavior.

THIS work is a new addition to the list of texts in introductory statistics for students of psychology. The somewhat cryptic title is borrowed from the concepts of information and noise in information theory and reflects the author's unifying theme that statistical analysis is a breakdown of total variance into portions assignable to measurable variables versus the unanalyzable ("chance") residual.

The scope is broad, probably too broad to cover in a one-semester course. In addition to the standard descriptive methods (central tendency, dispersion, correlation, and regression) and the standard inferential methods (the t

test, the *F* test, factorial design, and chi square) there is a fair coverage of nonparametric methods (29 pp.) and factor analysis (26 pp.). As a valuable novel addition, there are nine pages devoted to teaching the use of the slide rule.

The broad scope is supported by considerable depth in theory, in terms of both perceptual and mathematical meanings. The mathematics usually adhere well to the author's stated policy that "the underlying concepts are accessible to proof in terms of elementary college algebra, and anyone who has grasped these principles can learn to understand and apply methods whose development required a good deal of mathematical genius." The 260 pages of text contain ample concrete support from 36 figures, 52 tables of illustrative data, and problems at the ends of chapters. The appendices contain a glossary of the mathematical symbols used, selected formulas for review and reference, the probability tables needed for the inferential methods described in the text, three transformation tables, 10,000 random digits, tables of squares and square roots of numbers, and a four-place table of common logarithms.

Some of the features which distinguish this text from the plethora of current statistical texts have just been noted. An additional distinguishing feature is its fortunate revival of eta-squared and epsilon-squared as meaningful statistics on which to build conceptions of statistical analysis, an approach which apparently accounts for the title of the book. A further good distinguishing feature is the adherence to the separate meanings of "variance" (σ^2) and "variance estimate" (s^2), a usage that avoids a major logical nuisance created by some recent texts which define *variance* in terms of the estimate of the universe variance.

A different sort of distinguishing feature is the author's occasional light vein in his effort to make a tough subject more interesting. There is some risk that the entertainment is slightly overdone for serious students who like tough subjects and might object to chapter subtitles such as *The Rose-Tinted Spectacles* or fail to appreciate the *Third Interlude* of the book, entitled *A Bit*

of Poesy and consisting of two pages of metric verse on the standard error of a difference. The book is undoubtedly based on the author's own course and has both the advantages and disadvantages of a text which consists of a write-up of a particular instructor's way of teaching a subject. Its meaningful and theoretically sound approach, however, recommends it regardless of any of these idiosyncracies.

Psychopathology, Ltd.

Kurt Schneider

Clinical Psychopathology. (Trans. by M. W. Hamilton.) New York: Grune & Stratton, 1959. Pp. xvi + 173. \$4.50.

Reviewed by LEE SECHREST

who is Assistant Professor in the Department of Psychology at Northwestern University, having attained to his doctorate in clinical psychology a few years ago at Ohio State University under the tutelage of George Kelly and Julian Rotter. He likes personality assessment, psychotherapy, concept formation, and classroom learning and thinks of this broad spread as not inconsistent with his 'tough' training at Ohio State.

IF one can judge the refinement and success of a book by the editions it has been put through, this little volume by one of Europe's eminent psychiatrists is remarkable indeed, for it is now appearing in its fifth edition. Professor Schneider, who retired from his post of Professor of Psychiatry and Neurology at the University of Heidelberg in 1955, published the first edition of the present volume in 1935 as *The Psychology of Abnormal Feeling and Impulse*. Although one might expect a rather thoroughly sterilized, vacuum-packed production after so many editions, Schneider's present version has a remarkable freshness. The writing style, which is more in the nature of written lectures

than is usual for academic works, is an achievement of facility and grace. Miss M. W. Hamilton must be complimented on a sparkling translation.

The substance of Professor Schneider's position is that behavior pathology has its genesis in inborn, constitutional factors. He makes a rather sharp distinction, however, between the lesser anomalies of behavior and the psychoses, dividing behavior disturbances into two principal groups. Group I, which consists of abnormal intellectual endowment, abnormal personality, and abnormal psychic reaction, he entitles *Abnormal Variations of Psychic Life*. Of these he says that some somatic causation or involvement may be supposed to exist. Group II, *Effects of Illness (and Defective Structure)*, consists of what for him are the only true mental diseases, the psychoses. Schneider believes that, in addition to the obviously somatogenic psychotic reactions, schizophrenia and cyclothymia are true diseases, i.e., they have their basis in a morbid condition. Thus, he says: "Here we are firmly postulating that cyclothymia and schizophrenia are psychopathologic symptoms of some unknown illness. If 'postulate' seems too strong, we can say 'working hypothesis' instead" (p. 5). Schneider feels that our present ignorance is unfortunate but that it is inevitable that the morbid conditions responsible for cyclothymia and schizophrenia will be discovered. It is our loss that he chooses not to discuss in detail the arguments in favor of his somatogenic hypothesis.

Even though one may be repelled by the extremeness of this biological orientation, Schneider presents himself as a stalwart and stimulating thinker. Throughout the book he records his views in a reasoned and tentative manner. As a matter of fact, he is at times quite disarming with his admissions of the limitations of his position. The book is not the work of the thoroughgoing dogmatist so often met in this field. Its author has obviously made an honest effort to present his material in a scientific manner—though one may quibble occasionally concerning the clarity or success of his efforts. For example, he proposes the following "precise, scientific" definition of psychosis: "psychoses are only those psychic anom-

lies which fall into Group II, that is, they have a morbid character, which in our view may also be said of the effects of defective structure." The last laugh, however, may belong to Professor Schneider, for he reports that during a five-year period figures collected in his department of psychiatry at a hospital revealed only 35 doubtful cases from among 2754 diagnoses. Let that record be challenged!

ALTHOUGH *Clinical Psychopathology* is superficially of a kind with numerous other books on the same topic, its differences are striking. It makes no attempt to achieve the encyclopedic coverage usually encountered in textbooks. Thus its references to research findings and basic data are sparse. It has chapters on psychopathic personality, feeble-mindedness, organic brain diseases, psychoses, etc., but largely ignores the whole area which most psychologists have come to know as 'neurosis.' To be sure, Schneider points out that the term *neurosis* is meaningless, but for many readers that may not justify omitting discussions of compulsions, hysterias, psychosomatic diseases, phobias, etc., in a book on psychopathology. On the topics discussed, however, Professor Schneider is interesting, often unorthodox, yet usually eminently sensible. His attempt to classify psychopathic personalities seems worthwhile and realistic, and his discussion of symptoms of schizophrenic disturbance demonstrates masterful ease in this difficult area. It is refreshing to read the ideas of a man who can and has thought for himself.

Although Professor Schneider is a leader in the phenomenological school which has developed out of the ideas of Kraepelin, there is little in his book that is likely to alienate adherents of any other position. Aside from the strongly biological orientation, his point of view is marked by emphasis on form rather than content in psychopathology. He gives virtually no discussion of the content of symptoms. The constitution or its alteration by some morbid process provides the setting for the occurrence of symptoms whose specific content is to be attributed to the operation of personality factors.

Although this volume may be recom-

mended as a stimulating work, its scope and significance are limited, as would indeed be indicated by its size. The focus of the work is almost exclusively on diagnosis—that and nothing more. And the question inevitably must be asked, "To what purpose?" Certainly precision of diagnosis is important, but only insofar as it contributes to the making of decisions. In this work the implications of diagnosis are ignored:

The index, for example—which, by the way, is very inadequate—lists only two single-page references to *Treatment* and none to any other comparable term. Even admitting Professor Schneider's view that we must begin with the basic data of symptoms in our quest for understanding, one might wish that some consideration be given to the possibility of implementing decisions about patients.

Age Regression by Hypnosis

Robert Reiff and Martin Scheerer

Memory and Hypnotic Age Regression: Developmental Aspects of Cognitive Function Explored through Hypnosis. New York: International Universities Press, 1959. Pp. 253. \$5.00.

Reviewed by MARTIN T. ORNE and DONALD N. O'CONNELL

The senior author, Dr. Scheerer, is Professor of Psychology at the University of Kansas. He had his doctorate from the University of Hamburg and a dozen years of teaching and research in New York City before he went to Kansas. He is known best for his research on brain injuries, symbolic processes, and personality theory. Dr. Reiff, who is now Chief Psychologist at the Jewish Board of Guardians in New York City, took his PhD under Scheerer at Kansas in 1954, and the present volume, their joint undertaking, grew out of Reiff's doctoral dissertation. Dr. Orne, the senior reviewer, is a clinical psychologist and psychiatrist, at present Instructor in Psychiatry at the Harvard Medical School and Research Associate in Harvard's Department of Social Relations. He is director of the Studies in Hypnosis Project in the Department of Psychiatry. O'Connell is a research associate with the Studies in Hypnosis Project at the Massachusetts Mental Health Center and is working now on the problem of hypnotic age regression.

THIS monograph consists of two separate sections. The first considers certain theoretical aspects of memory and related functions. The second section reports an experiment based

on these theoretical aspects. These two parts are so disparate and of such different quality as to justify their separate review.

The chapters on theory present a detailed integration of dynamic theories of memory and the genetic theories of cognitive development characteristic of Piaget and others. The traditional concept of memory as an isolated capacity for retention is briefly discussed and compared with the Gestalt view, which treats memory as a dynamic function that is intimately related to cognitive-perceptual field organization during learning and recall and to the trace field during retention. The reviewers recognize that the trace theory of memory is not the strongest area of Gestalt theory, especially when it deals with the temporal organization of trace systems. Nevertheless, these chapters pose a thoughtful, convincing argument and review succinctly the relevant literature.

Some clarification is found in the distinction that the authors make between *remembrances*, memories experienced with a personal-temporal quality, and *memoria*, memories which do not have an autobiographical index quality. *Memoria* include, on the one hand, general items in the information repertoire of the individual, automatized habits, and so

forth, and, on the other hand, the schemata of cognitive functioning at *each stage* of development. This latter characteristic is particularly relevant to the further argument. For instance, the difficulty that an adult encounters in his attempts to recall childhood events is assumed to be the result of the qualitative differences between his present adult modes of thinking and those used previously in childhood. The events of the past are learned in the context of the modes of thinking characteristic of the past. As the individual proceeds from autistic to egocentric to realistic and rational modes of thinking, each preceding system of schemata is, in turn, forgotten. And forgotten with them are more and more of the events contextually associated with them. In effect, it is argued that we cannot remember childhood events because we can no longer think as we could when children. If those earlier modes could be revived in some way, it is assumed that recall for events contemporaneous with them would be greatly facilitated.

Some of the more recent psychoanalytic views and ego-psychology approaches, notably those of Hartman and Rapaport, are shown to be consistent with this theory. Finally, the authors suggest that hypnotic age regression is suggested as a method of demonstration, and the literature on that subject is reviewed.

THE second part of the monograph is devoted to experimental demonstration of age regression. Five highly trained hypnotic subjects, who made up the experimental group, were chosen from a large population on the basis of their facile hypnotizability. Each member of the group was 'regressed' successively during the experiment to the ages ten, seven, and four. After every regression each was brought back to his actual age, and amnesia was induced for the previous experience. The behavior of these subjects under hypnosis was compared with the behavior of a group of fifteen individuals each of whom was asked to play the role of *one* of the three ages. The groups were compared in terms of mode of functioning in specific task situations, with a view to establishing the developmental stage of each indi-

vidual. The hollow tube test, exploration of how the subject tells time, understands right from left, does simple arithmetic, and so forth, were used, in the style of Piaget and Werner. A free play period, word association tests, and a recitation of the Pledge of Allegiance were also used. Here the authors point out that they were not so much concerned with the success or failure of the task as with the particular mode of thinking. The differences in perform-



—Constance Scheerer Photo

MARTIN SCHEERER

ance between the two groups, analyzed in terms of these developmental criteria, were attributed to the effect of the hypnotic age regression. In addition to the tasks, each subject was presented with a number of factual questions during regression, dealing with such things as the day of the week on which his tenth birthday fell, the names of his schoolteachers at various ages, and the names of the classmates sitting next to him. Finally, there were a number of experiments reported on the revival of forgotten skills. For instance, one person was able to translate a Latin passage which she had read in her second year of high school, and for which she denied understanding in her waking state. These final experiments deal with single examples, and no control groups were used.

The authors conclude from the set of experiments using both simulated and

hypnotized subjects in age regression that support is found for both the development-stage theory of memory and genuineness of hypnotic age regression. The ingenuity of their method and the potential importance of their findings make the monograph an important contribution to our understanding of this subject. It is thus particularly unfortunate that the authors have not avoided several serious methodological defects. They properly recognize the limitation of using a small sample, but there are more serious difficulties.

OF the five subjects used for hypnotic age regression, each was regressed to all of the three ages. The authors conclude that this is *equivalent* to the three groups of five subjects, each group regressed to one of the ages. They argue that the amnesia induced after each session precluded contamination.

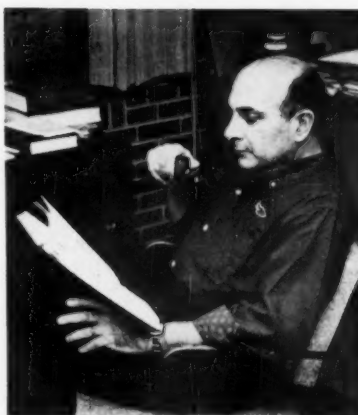
That conclusion is not supported by the work of previous investigators. In one study Strickler showed that, although subjects denied recollection of trance events after induced amnesia, there was a 50 percent saving for both nonsense material and image learning when the 'forgotten' material was presented for relearning (Hull, 1933). Further, it has been generally observed that subjects will use information gained in hypnosis and 'forgotten' after induced amnesia if a solution to a relevant problem is called for, but will deny recall when asked directly. The simple fact that a post-hypnotic suggestion will be followed despite amnesia for the suggestion demonstrates this special quality of the hypnotic amnesia. Introspective recall is prevented, but there is no interference with the suggested performance. The subject is motivated to comply to the best of his ability with the suggestions of the hypnotist. In his classic paper on unocular blindness, Pattie has shown the extent to which some subjects will go in attempting to carry out the suggestions (1935). It would seem incongruous to assume that just because amnesia has been suggested for events occurring previously in trance the subjects would not utilize the knowledge gained to improve their performance on subsequent occasions.

Moreover, the authors say that the

actual subjects had had some experience with age regression, before the actual study, to see whether they were able to follow this suggestion. Thus the control simulators employed in this study had only one opportunity to play the role of age regression whereas the hypnotic subjects had at least three and usually several more opportunities to learn this kind of task. For instance, the authors' data show that the simulators did far better at simulating age ten than the younger ages. This is the age to which the hypnotized subjects were regressed first. It is therefore difficult to judge how much of the discrepancy in performance between the simulators and the hypnotic subjects is due to practice in and information about the tasks and how much is due to the actual phenomenon of age regression. For the differences between simulating and regressed subjects to have clearer meaning it would have been necessary to utilize either three groups of hypnotized subjects regressed to one age or one group of simulators regressed to all three ages.

A further objection may be made that no controls were used for these tests of subjects who were in the hypnotic state but not age-regressed. The possibility that an increase in autistic thinking might be an intrinsic quality of the trance state should be considered. If such be the case, the hypnotically age-regressed subjects would appear to be functioning at an earlier developmental level, for autistic thinking is also a characteristic of the earliest stages of cognitive development.

THE authors emphasize that the behavior of the simulating subjects was unconvincing during the period of free play, but here both the simulator and the hypnotist were aware that simulation was involved. Under these circumstances the experimenter may not treat the simulating subject as he would the hypnotic subject, despite identical wording of instructions. Also for the subject who knows that the experimenter is aware of his simulation, the task is onerous and difficult. He knows that he is only playing a game and feels embarrassed about really throwing himself into the requested role. In our experi-



—Constance Scheerer Photo

ROBERT REIFF

ence simulators perform very much as described by Reiff and Scheerer. When the situation is designed, however, to leave the experimenter in ignorance as to which subjects are simulating, and when in addition the subjects are told that their task is to deceive him, then an entirely different order of performance is obtained. Under these circumstances real and simulating subjects, having been treated identically, tend to become behaviorally indistinguishable.

In this context, we note that another control is lacking. No data are presented on the performance of actual children of the appropriate ages. Piaget reports data, but they were obtained in another country, by different experimenters, and in vastly different settings. It would seem necessary to obtain some normative values before one could comfortably use the procedures as a test. While some of the word association tables for children are relatively recent and available, it would seem important for the authors to have obtained comparative information directly from control groups made up of children of the appropriate ages.

One final and serious drawback for a monograph of this type is the failure to present a meaningful rationale for hypnotic age regression. One would hope to find a discussion relating the phenomenon to contemporary personality theories. However, the reader is left with the impression that hypnotic age regression is somehow magically achieved by requesting a hypnotized subject 'to be' a given age.

It is possible to see this study in either one of two ways: as an investigation of the schemata of thinking utilized by the child at various ages and their relationship to memory, using the approach of hypnotic age regression; or as a study of hypnotic age regression utilizing the developmental tests of Piaget and Werner as the method of study. The authors appear to be trying to achieve both ends and the reader is never certain as to their primary intent. The demonstration of genuineness of hypnotic age regression is methodologically weak and thus throws some doubt on the use of the phenomenon as a valid means of investigating the schemata of thinking employed during the development of the individual. Seen as a study of hypnotic age regression, the use of developmental tests is an ingenious and appropriate technique, but here the absence of data on children's performances becomes a serious omission. Thus while the theoretical contribution is provocative and the ideas underlying the empirical study are worthwhile, the absence of adequate controls leaves the authors' findings as hypotheses that await further testing.

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Psychology labours under the great difficulty that the worker in it cannot, like other men of science, publish his conclusions as discoveries which will necessarily be accepted by any persons competent to judge. He can only state his conclusions and his reasoning and hope that they may gradually gain the general approval of his colleagues.

—WILLIAM McDUGALL



CP SPEAKS

AUTHORS' EGOS

Is an author biased? Of course, for he is a human being with ego-involvement in respect of the precious book he has just written and managed to have published. He wants it to be good and to have the readers say that it is—his readers, the readers of *his* book. Indeed authors may be expected to be somewhat more biased than reviewers because they have more at stake. The author has achieved identification with his product. The reviewer comes at a new book—ordinarily he does—as a neutral.

Yet reviewers are idiosyncratic with individuality arising from their special attitudes and values, their prejudices. CP has long been trying to accustom its readers to the thought that reviewing must inevitably be idiosyncratic, and that the objective 'truth' about a book can not be found instantly but emerges gradually by way of criticism and especially by debate between honest men with different values, by what CP calls the tennis of dissent to dissent to dissent.

Now here comes Dr. W. S. Taylor, one of Smith College's psychologists, with an article in *Scholarly Books in America* (May-July 1960, 2, 4f.), who says:

A recent book in motivational and abnormal psychology received reviews which were not only variously favorable and unfavorable, which is to be expected, but differed greatly on most aspects of the work. Thus one reviewer found it dated; another found it up-to-date. One thought it not dynamic enough; another liked the dynamic approach. For one, there was no clear organization; but for his opposite number, the organization was deeply satisfying. For some, the style was wretched, and for others, excellent. Several considered the book poor for beginners; others, good. Likewise, for advanced students, the judgments ran from poor to good.

CP does not think that the judgments of books revealed in its pages vary quite so widely in general, although every now and then its department of dissent, ON THE OTHER HAND, discloses a wide disparity of judgment about a particular book and the review of it, a difference of view that occurs most often, yet not always, between the author and his reviewer.

Dr. Taylor suggests that this kind of disparity would be greatly diminished if the book's author were given some space immediately after the review, in order to comment upon what the reviewer had said. (This idea Taylor got from M. R. Ernst and Alexander Lindey in *Hold Your Tongue!*, Wm. Morrow, 1932.) Dr. Taylor thinks that the prospect of such counter-argument might make the idiosyncratic reviewer more careful in his pronouncements, and CP thinks that it would start the cybernetic tennis of dissent to dissent to dissent. But for how many reciprocal dissents would you hold up publication of the review? CP is slow now in giving its eager readers the latest news about the speeding progress of thought. Would they wish to wait a few months longer to get the full resumé of the battle of the giants, or do they prefer the attack-by-attack account as they get it now in ON THE OTHER HAND?

A quicker and, CP thinks, a more hopeful method would be Dr. Taylor's alternative of having author and reviewer correspond before the review is printed in order to iron out inaccuracies and to make sure that the reviewer has not made errors that he would admit if they were pointed out by the author. This procedure would, however, still cause delay, for the correspondence would hardly be limited to a single exchange. It would also run into another human idiosyncrasy. Often a writer does not believe in arguing things out. He

does his best, he lets his critic do his best, and then he sits back to let the public—or perhaps posterity even—decide, provided it is interested. Such a writer's philosophy is simply that he does not believe that either he or his critic—each committed to a view, one of which is irrevocably bound in hard covers—can profitably undertake debate. The language may remain urbane but the thinking is prejudiced. It is better to leave this decision to others—and more dignified. So thinks this kind of an author, a wise psychologist, who recognizes that his own ego-involvement disqualifies him for judgment.

This comment brings us back to our starting point. Can an author be unbiased enough to defend himself with profit to the intellectual enterprise that is 'progress'? It depends, thinks CP, on the emotional maturity of the writer, on his capacity to suspend judgment when evidence is conflicting, on how passionately he wants to be objective about himself, and perhaps also on his ability to perceive his own biases as humorous. CP was early warned against starting ON THE OTHER HAND, its department of dissent, but has stuck to it and has not weakened yet. It is better than the more usual practice of leaving the injured author defenseless. Even prejudiced published dissent warns the reader not to be too complacent in accepting the propaganda that CP of necessity prints. Never do we get rid of prejudice, which is part and parcel of every personality. There is no such thing as a living human being without attitudes. Posterity too will have its values, but at least it will have less ego-involvement as it considers our present-day problems.

Meanwhile it is worth while noting, in CP's here and now, that the most usual dissenter to a reviewer's idiosyncrasy is the criticized author himself, the man whose ego is most deeply involved. It could be argued that the right of dissent be denied to authors. Anyone interested in truth, sound logic, accurate facts, the progress of knowledge could be accounted free to dissent from a review only if he has no personal identification with the book that is being depreciated. No psychological research would be tolerated if it ad-

mitted, without control, such a bias in the selection of its subjects. This is not, however, CP's rule. CP says in this context: Do not ask too much of human nature. Prejudice (attitude) is the weft of personality.

So let the reviewer do his best, though his review be idiosyncratic. Let the objecting author resolve to reply objectively, though he knows how reason yields to the ego. Human nature has always had these biases, and progress in civilization has occurred in spite of prejudice. An imperfect instrument can be made to work, and often there is none better to use.

BOOKS TO COME

SOMEWHERE along around next September, sooner or later, Wiley should be bringing out a book under the title, *The Psychology of Judgment and Choice: a Theoretical Essay*, by Frank Restle, a Stanford PhD who is now at Michigan State University. Restle is laying down half a dozen firm hypotheses that do not fit the current Zeitgeist at all, hypotheses in which he believes strongly and which he explicates and defends, with the hope of stirring up the kind of dissent and argument that ultimately ends in assent, at least in the assent of posterity if not, as he hopes, in the assent of contemporaneity. CP cannot here venture into the explication of the nature of these assaults on the community of current opinion. Next September is soon enough for you to get as angry as Restle hopes you will.

SOMETIME this spring Holt, Rinehart and Winston, as they are called now, will issue under the editorship of Dr. Sol Saporta a volume called *Readings in Psycholinguistics*, consisting of 43 selections by 52 authors. The contributions are about equally divided between linguistics and psychology. The better known names among the psychologists are J. B. Carroll, Davis Howes, K. S. Lashley, G. A. Miller, C. E. Osgood, and B. F. Skinner. The project was initiated by the Committee on Linguistics and Psychology of the Social Science Research Council. Saporta, with a doctorate in Spanish linguistics from the University of Illinois, is spending the current year at the University of Wash-

ington, on leave from Indiana University where he is assistant professor of Spanish and Linguistics.

KHRUSHCHEV'S BENISON

VERY slowly CP's letter writers fall in line to send CP letters with no ad hominem in them. The reviewers nowadays meet this requirement pretty well, and the letter writers are improving. CP's pages are not (CP feels sure) the place to discuss the integrity or intelligence of any author or reviewer or other letter writer, to discuss them directly or by innuendo. There's a way of writing "Jones announces . . ." instead of "Jones says . . ." that implies that the writer has access to Jones' emotional life, when it is much more likely that the sure thing in the writer's cognizance is his own emotion.

So often it has been said that the abnormal teaches us to understand the normal. Last fall everybody watched the abnormal use of the ad hominem in the United Nations and saw how personal derogation prevents comprehension and intellectual progress. So please stop sending in your shoes with your letters, your shoes with which to pound on CP's table.

If you do not know, Reader, of what CP speaks, that is because you have not seen the letters before they were published. And in this respect CP is truly grateful, for there has yet to be a table-thumping letter writer who has balked at playing CP's game at recasting his letter with the ad hominem out. Remember then Mr. K. at the U. N. and turn always to the main task: advancing the knowledge and understanding of CP's readers.

OMNIUM-GATHERUM

YOU can and always could buy M. Pei's *Language for Everybody*, Pocket Book GC 44, 1958, 340 pp., reprint, for only fifty cents (only \$.50). Why is this news? Oh, because CP told you erroneously (Oct. 1960, 5, 334) that it cost seventeen times as much. But in general you had better keep on believing CP; it does pretty well.

—E. G. B.

Small Wares for Private Practice

Theodore H. Blau

Private Practice in Clinical Psychology. New York: Appleton-Century-Crofts, 1959. Pp. x + 184. \$3.00.

Reviewed by STANLEY G. ESTES

who, after a Harvard PhD and long years of teaching at Northeastern University and at Harvard, has now for more than a dozen years been in private practice in the Boston area. Just now he is immersed in psychological counseling of adults at the professional level. He is a trustee of the American Board of Examiners in Professional Psychology.

NOWHERE in the literature of psychology, Dr. Blau says, is there "a critical yet constructive analysis of private practice." It is his purpose to meet this lack by presenting "an organized series of observations on the individual and responsible practice of clinical psychology." These observations are based on his own experience of six years in private practice and on the scrutiny of the case records of 1200 persons seen by him and two other clinical psychologists during a period of three years. Dr. Blau received his PhD in 1951 at Pennsylvania State University and since 1953 has been executive vice-president and director of the clinical division of Byron Harless and Associates in Tampa, Florida.

The book is addressed explicitly to psychologists not engaged in private practice for whom, in consequence, "the conditions of individual practice . . . in a community setting is a professional mystery." Among these is "a fairly large group of intelligent and capable psychologists without any experience or direct observation of private practice settings [who] insist that only charlatans, money-grabbers, and unethical psychologists would consider operating in private practice." The needs of two other groups of readers continually capture Dr. Blau's attention, interrupt-

ing and obscuring the description and justification of private practice that he is addressing to the first group. Thus we have three books mixed but they are not blended in one.

The second of these books, suitable for undergraduates, is an occupational information pamphlet setting forth the formal educational, training, and professional accreditation requirements for the practice of clinical psychology and supplying such details as the address of the secretary of the American Board of Examiners in Professional Psychology.

The third book is a manual for the enterprising, energetic, young psychologist on how to begin and to maintain successfully an ethical private practice. The first part of this book deals in detail with what might be called installation and operation: how to rent and equip an office, select a secretary (don't use your aunt or niece; get someone with intelligence, good judgment, and the necessary skills), how to be listed acceptably in the classified section of a telephone directory, how to get established with members of closely related professions and become well known in the community, how to go about setting up a group private practice. The second part of the book deals in a cursory and didactic way with the skills, procedures, knowledge, and distinctive pitfalls involved in the essential professional functions of the clinician: evaluation, treatment or disposition, and reporting.

THE descriptive analysis which Dr. Blau gives of his own practice is comprehensive and vivid, although the exhortatory and evaluative language in which much of it is cast is inappropriate for the readers whom he is formally addressing. In justifying the private practice of clinical psychology he says he will "have to meet the objections of some of his colleagues who feel that the knowledge from psychology as a science is insufficient to support psychology as a profession." He offers no such justification, but makes its existence an article of faith. As a further justification for private practice he advances the statement that clinical psychologists have "the most complete training in human behavior as related

to growth and development." If these statements are true, they still do not give a distinctive justification for private practice. A third justification offered is the great need of communities for professional help in the prevention and cure of mental miseries.

The book is a distinguished piece of bad writing. It is muddily conceived, strewn with needless and imprecise words, spotted with broad references, dangling participles, misleading and awkward constructions. Here are some examples. "When the data have been collected, the patient has been seen, observations made, and tests given, this

material must be in some way made operational to deal with the patient's problems" (p. 50). "Assumptions are more subject to invalidity and bias than facts, and it is for this reason that a series of tests and observations are often administered" (p. 52). "The psychologist must avoid situations where he treats restlessness, irritability, poor attention span, and lowered school grades with play therapy, when the child is reacting to pinworm infestation, or endocrinological imbalance" (p. 85). "Medical practitioners as a group are fairly unfamiliar with psychology, much less clinical psychology" (p. 84).

The Psychology of Set Theory

Léo Apostel, A. R. Jonckheere, and Benjamin Matalon

Logique, apprentissage et probabilité. (Etudes d'Épistémologie Génétique, VIII.) Paris (108, Boulevard Saint-Germain): Presses Universitaires de France, 1959. Pp. 186. 900 fr.

Reviewed by PATRICK SUPPES

As contributors to the Études d'Épistémologie Génétique the authors place themselves in the important and influential group stimulated by the thought of Jean Piaget in Geneva. Apostel is a professor of philosophy in Belgium. Jonckheere lectures on psychology in University College, London. Apostel and Matalon contributed to the third and fourth Études (CP, Oct. 1958, 3, 310-312). Dr. Suppes, the reviewer, is a logician and philosopher of science at Stanford University. He has published Introduction to Logic (1957), Axiomatic Set Theory (1960), with Donald Davidson, Decision Making (1957), and with Richard C. Atkinson, Markov Learning Models for Multiperson Interactions (1960). He is pretty well psychologized from being a Fellow at the Center for Advanced Study in the Behavioral Sciences (1955-1956), from association with W. K. Estes, and from his interest in learning theory, theory of measurement, and decision making.

THIS book is Volume VIII in the series *Études d'Épistémologie Génétique*, published under the direction

of Jean Piaget, and many of the themes in the present work are familiar from earlier volumes in the series. As is the custom in this series, the volume is not written jointly by the three authors, but actually consists of three separate articles, by far the longest of which is the first one by Apostel (138 pp.). It is entitled *Logic and Learning*, and from casual references in later volumes I would judge Piaget thinks highly of it as a theoretical contribution.

The task Apostel sets himself is an exciting and interesting one. He wants to give a behavioristic account of the logical notions of class, relation, proposition, and inference, as well as of the logical constants like *and* or *or*. He begins (Part I) by attempting to provide psychological definitions of these five notions. For example, a class exists for a subject if, roughly speaking, he makes the same response when presented with any member of this class, but does not make the response to any object not in the class. This is not exactly what the author says, but one of my difficulties is that I cannot understand precisely how the author does want to define the

psychological existence of classes (or relations or inferences). After a promising beginning, Part I is disappointing. I doubt that it would be clear to anyone how to design an experiment to test the intuitive adequacy of the definitions proposed.

In Part II Apostel gives an admittedly routine survey of the 'laws' of learning. What he does is to sketch the theories of Guthrie, Tolman, Piaget, and Hull, in that order. His intention is to provide background for the theoretical analysis in Part III of the definitions introduced in Part I. What is surprising is that he tries to bring together all four of these theories in terms of the linear response models of Bush and Mosteller. Unfortunately the attempts to extend these models to the theories in question are too qualitative and vague to be regarded as serious contributions to the development of the theories or the literature of mathematical models.

For the same kind of reasons Part III also fails in its mission, but some of the discussion centering around the application of Hull's theory to classificatory behavior is interesting and worth reading. The same thing cannot be said for the section which gives a psychological interpretation of axiomatic set theory. I would characterize this discussion as fanciful. To give two examples, the author claims that the technical distinction between sets and proper classes in von Neumann set theory (which does not exist in Zermelo set theory) is reflected in the classes defined psychologically. Secondly, the psychological interpretations given the axiom of infinity and the axiom of choice are in reality scarcely connected with their literal mathematical meaning. The axiom of infinity is said to be 'verified' to the extent that new classificatory divisions can be constructed without limitation. This is perhaps close to Aristotle's idea of the potentially infinite, but this kind of infinity is already present in set theory without the axiom of infinity, as was pointed out by Zermelo in 1909 when he showed that elementary number theory can be constructed within set theory without this axiom.

The important point highlighted by the analysis of set theory is that Apostel tries to give a psychological interpreta-

tion of logic and the theory of sets without any intervening general analysis of language and meaning. This would seem to be clearly a mistake. Logic and set theory are the end products of a long and sophisticated verbal development. It seems unlikely that a seriously detailed behavioristic interpretation of them can be given in terms of nonverbal responses. The connection of set theory with the empirical world, let alone the behavior of organisms, is at best highly tenuous and indirect.

THE two shorter articles, one by Jonckheere and the other by Matalon, discuss various aspects of stochastic models for learning. Jonckheere gives a succinct review of the usefulness of stochastic models in dealing with experiments in which one of two responses is possible. He then goes on to propose an urn model which generalizes the usual linear response models. Actually I have myself never really seen the point of urn models in the present context. They provide one simple realization of discrete probability processes, but it is not a realization which is highly suggestive in terms of psychological theory. Jonckheere is unduly pessimistic in his discussion of the problems of parameter estimation and goodness-of-fit tests for stochastic models. When linear response models are replaced by stimulus sampling models, simple maximum-likelihood estimation procedures are often available. And the difficulty of non-independence of observations in applying goodness-of-fit tests disappears if transitions in responses rather than responses themselves count as the observations.

Matalon analyzes sympathetically certain properties of current stochastic models which give rise to problems—for example, their very restricted capacity for incorporating memory. He also makes some suggestions for extension but does not work out the details.

It should perhaps be remarked that none of the three articles seriously considers relevant experimental data.



Instinctive theorizing, when a fact looks to the eye as the eye likes the look.

—ROBERT BROWNING

A Case of Hypnotherapy

Fredericka F. Freytag

The Hypnoanalysis of an Anxiety Hysteria. New York: Julian Press, 1959. Pp. xiv + 412. \$6.50.

Reviewed by ARTHUR JENNESS

The author, Dr. Freytag, is a practicing psychiatrist of twenty years' standing since her training in the College of Medicine of Ohio State University. She is a member of the American Medical Association, the American Psychiatric Association, the Academy of Psychosomatic Medicine, and of both the older and the brand-new societies for the study of the clinical use of hypnosis. Milton H. Erickson praises this report in a foreword to the book. The reviewer, Dr. Jenness, is Professor of Psychology at Williams College and Chairman of its Department of Psychology. After a Syracuse PhD, partly induced by Floyd Allport, he was fourteen years at the University of Nebraska and now sixteen at Williams. W. R. Wells tutored him in hypnosis; he collaborated with Clark Hull in hypnotic research; he wrote the chapter on hypnosis in J. McV. Hunt's *Personality and the Behavior Disorders* (Ronald, 1944).

A PHYSICIAN on parole from a sentence for narcotic addiction has overcome the drug habit in a year and a half of psychoanalysis while in prison, but now agoraphobia prevents his driving a car. So a Miss X, an assistant in the hospital where he is allowed to practice his specialty, acts as his chauffeur. Becoming desperate, he consults a female psychiatrist, whom we shall call Dr. Y; she hypnotizes him and removes his fear of driving by the fourth session.

Dr. Y, however, discovers a castration complex that requires about seventy-five more sessions (mostly hypnotic) to overcome, not to mention countless hours of concurrent practicum with Miss X. Under coaching received in Dr. Y's office, the patient utilizes

Miss X as a sexual sparring partner who proves to be remarkably durable and genial despite the monotony of bouts terminating in *ejaculatio praecox* or less. Dr. Y demonstrates to the patient under hypnosis that Miss X is *not* his mother; intercourse with Miss X is therefore *permitted*. Misgivings about the priest (and presumably the probation officer) sharing this permissiveness are easily exorcised. The patient's repeatedly expressed wish to kill women during intercourse and Miss X's susceptibility to subarachnoid hemorrhages are "calculated risks." The story ends with the patient's unconscious revealing to him the true nature of his castration complex, so that he is able to fornicate successfully and without guilt.

'Psychiatric novels' have succeeded with poorer plots. (Not even Gibson's *The Cobweb*, which has about everything, contains a lady hypnotist.) But this is no novel. It is a serious and competent report of what the title covers, though *hypnotherapy* would be more appropriate than *hypnoanalysis*; there is much direct suggestion to the patient's unconscious with the stipulation that his conscious mind will not remember it. And presumably, fornication is not an end in itself, but only a means.

DR. FREYTAG has not attempted to deal primarily with behavioral theory. Though she refers to Bernard Gindes, to Milton Erickson and her other teachers, and to three standard psychoanalytic texts, she gives no indication of such recent predecessors in hypnoanalysis and hypnotherapy as Schilder, Rosen, Brenman and Gill, Wolberg, Lindner, and Kline. What she has contributed is a good workmanlike report of a particular job that well demonstrates what a combination of distributive analysis and hypnotic treatment can accomplish.

This is not a 'how-to-do-it' manual, though Dr. Freytag has added substantial rationale, comment, and interpretation to what she designates as verbatim reports of the transactions of patient and therapist. This report should enable the reader to gain a clear understanding of the plan and procedure, but he will have to evaluate for himself the principal superiority claimed for this form of therapy, viz., that it brings

about "marked favorable and often complete changes in feeling," even though the patient may be 'intellectually' unaware of the full nature of his conflicts.

Specifically, Dr. Freytag holds that this type of hypnotherapy, compared with other forms of deep therapy, (1) saves time by introducing anxiety-provoking material during hypnosis, thus eliciting the appropriate feeling immediately, and (2) makes the process relatively painless by inducing amnesia for the disturbing material between sessions. One means of "forestalling and preventing anxiety" by suggestion under hypnosis is this:

You are doing very well and you will make further excellent progress. It may not be necessary for you to know all the dynamic mechanisms producing the phobias. Your unconscious mind will know and that will be adequate for recovery (p. 27).

Censorious critics might attack the procedure in this particular case on ethical and moral grounds. Considering the "calculated risks" involved, one may hope that Freytag's paradigm will not be adopted as a model for practice by hypnotists other than licensed physicians. Yet this caveat should not depreciate the scientific value of her case report.

Absolutists and Pragmatists Save the Republic

Morris Janowitz

The Professional Soldier: A Social and Political Portrait. Glencoe, Ill.: Free Press, 1960. Pp. xiv + 464. \$6.75.

Reviewed by M. BREWSTER SMITH

The author, Dr. Janowitz, is a sociologist at the University of Michigan, with war-time experience in psychological warfare and propaganda analysis and nowadays a special research interest in political sociology. He is author of *Sociology and the Military Establishment* (Russell Sage, 1959) and, with Bruno Bettelheim, of *Dynamics of Prejudice* (Harper, 1950). The reviewer, Dr. Brewster Smith, is Professor of Psychology at the University of California in Berkeley. He is editor of the *Journal of Abnormal and Social Psychology*, recently president of the *Society for the Psychological Study of Social Issues*, and habitually concerned with problems of social psychology and the psychology of personality. He was co-author with Samuel A. Stouffer and others of *The American Soldier* (Princeton Univ. Press, 1949).

TO the intrinsically civilian world of American academic scholarship, the ascendancy of the military establishment as a salient and enduring fact has been

difficult to digest. Psychologists and social scientists have not been allowed to ignore it, if only because of the heavy involvement of the services in our post-war patterns of research support; but only recently have social scientists given much research attention to civil-military relations, which is the focus of this competent and judicious book.

The 'professional soldier' with whom Janowitz is concerned is the regular Army, Navy, or Air Force officer, and particularly the elite group of generals and admirals whose ways of playing their organizational roles have decisive policy significance. How have the role conceptions and operating assumptions of these military leaders been shaped by selective recruitment, indoctrination, and distinctive military experience over the half century that spans the aftermath of Indian fighting and the present technically developed organization poised globally in permanent quasi-alert? As military decision and national policy become increasingly interwoven, how do the thought-ways of military

leadership come to bear on national decisions? How does the principle of civilian control work out in practice, and how can it be strengthened?

As a policy-oriented sociologist and political analyst, Janowitz uses flexible methods that bring into view more kinds of data than a psychologist concerned with erecting a bit of theory on a tidy empirical basis would be likely to allow himself. The centerpiece for his analysis, to be sure, consists of systematic career data on officers of general (or admiral) grade in the peacetime years of 1910, 1920, 1935, and 1950. Further systematic data are derived from a 1954 questionnaire survey of Pentagon staff officers, and from intensive interviews in 1958 with over 100 colonels and brigadier generals (or the equivalent) on duty in the three service headquarters. But Janowitz also relies heavily on historical, documentary, and biographical sources, which he handles with apparently secure scholarship, to develop and illustrate his themes. Much of the interest of the book comes from these concrete and 'gossipy' details. In the nature of this kind of enterprise, one cannot assign *p* values to particular propositions; the over-all picture of historical and contemporary trends nevertheless carries considerable conviction. Up to his final epilogue of policy recommendations, Janowitz effectively maintains an objective tone.

CLEARLY this book is not 'psychology' either in subject matter or in approach, although social psychological issues are obviously involved. Yet many psychologists will find it worth a careful reading. Those whose work brings them in contact with the military will find it a generally reliable orientation in depth to military thinking. And the growing number who are puzzling at ways of bringing psychological competences to bear on the momentous issues of national and international policy may gain from it a valuable corrective to their professional blind spots. Here is some of the social, historical, and political context we had better keep in mind if our techniques, variables, and formal models are to be relevant.

Central to the book is a typology of military leadership that Janowitz ex-

amines with respect to both antecedents and consequences. Specimens of the traditional type of 'heroic leader,' characteristic of the days of the punitive expedition, are still around and continue to play an essential role in the military self-image. But predominantly the top leadership belongs to the newer type of 'military manager.' Among the managers, dispute polarizes between those committed to an 'absolutist' conception of the military mission (victory, 'massive retaliation') and the proponents of a more 'pragmatic' view that sees military means in the context of broadly political ends and consequences (e.g., 'graduated deterrence'). The former, 'absolutist' orientation goes with tactical command experience, preoccupation with a prestigious weapons system, and World War II affiliations with the Asian Theater and the MacArthur tradition; 'pragmatists' are bred in staff experience, in affiliations with the European Theater and the Marshall tradition, and in the services of declining combat importance. Pragmatists predominate in the Army, absolutists in the Air Force.

The myth of a monolithic 'power elite' dissolves under Janowitz's scrutiny. Despite the convergence in role between the military and the industrial manager, Janowitz presents persuasive evidence that the integration of military and industrial leadership is considerably looser than C. Wright Mills' image would suggest. And on the part of the military there is the endemic conflict within and between the services, which Janowitz succeeds remarkably in making intelligible to the lay civilian.

At the end Janowitz comes out for what he calls a professional constabulary role for the military establishment, rooted in the 'pragmatist' position, but psychologists will be less interested in this advocacy than in the detailed and seemingly accurate "social and political portrait" that comprises the body of the book.



If truth were not often suggested by error, if old implements could not be adjusted to new uses, human progress would be slow.

—OLIVER WENDELL HOLMES, JR.

Predicting the Child's Behavior

Sibylle Escalona and Grace Moore Heider

Prediction and Outcome: A Study in Child Development. (Menninger Clinic Monograph Series, No. 14.) New York: Basic Books, 1959. Pp. xvi + 318. \$6.50.

Reviewed by JEROME KAGAN

who is Chairman of the Department of Psychology at the Fels Research Institute at Yellow Springs, Ohio. He has a Yale PhD where he worked with Frank Beach on animal mating behavior. Now he is concentrating on the biological and social forces that direct human development. He has a book coming along which summarizes the characteristics of the members of a group of adults from birth to now and shows that they changed less than you'd think.

THIS Monograph of the Menninger Clinic describes a unique research exercise in child development and postulates several key dimensions that might provide some order to the puzzle of infant behavior. The empirical effort is an attempt to predict the functioning of a preschool child from a four-hour infant observation, made between one and eight months of age, and some information on the parents. The infant material was gathered during the late 1940s (with Dr. Mary Leitch) when Dr. Escalona was Director of Research at the Menninger Foundation. She is now Professor of Psychology at the Albert Einstein School of Medicine. Dr. Heider has been with the Foundation since 1949 and was involved in the collection of the infant data.

Armed with the observations on 31 infants and the descriptive material on the family, Escalona made over 1,000 predictions of behaviors she expected these children to show when they were between three and six years of age. The prophetic statements differed in level of inference and ranged from gross activity level (Sandy will be an active, vigorous

person) to perceptual reactions (Raymond will show sensitivity to sensory stimuli) to social behavior (Martin will have a sociable disposition). The criterion information on the preschooler included behavioral observations, psychiatric play sessions, projective test protocols, and interviews with the child's parents.

The principles that gave direction to Escalona's predictions included several loosely formulated hypotheses regarding the consequences of perceptual sensitivity and quality of motor expression during the early months of life. Highly variable activity levels and explosive reactions to stimulation were viewed as diagnostic of unusually strong internal needs or an inability to inhibit urges to action. This defective control of motor discharge presumably impairs both the ability to delay gratification of strong motives and the capacity for abstract thought.

A second hypothesis involves the perceptual apparatus. Sensitivity to sensory input facilitates the growth of empathy and the ability to enter into rich and elaborate fantasy excursions. Finally, the infant's postural freedom and security in bodily orientation in space are assumed to influence the expansiveness of the older child's play.

These developmental hypotheses are exciting notions that invite systematic attention. The research neither supports nor refutes them.

THE heart of the text describes the evaluation of the correctness of Escalona's predictions. In a first strategy, Escalona and Heider independently studied the follow-up material and judged the degree to which the preschool behavior corroborated the predictions. The reliability of these independent judgments was quite low (48% agreement). When the original predictions were classified as either predominantly correct or incorrect, 66% fell in the former category.

Adoption of sex-appropriate behaviors, motor development and coordination, pattern of the child's interests, and capacity for attention and concentration had a high degree of predictive validity (over 80%). Achievement needs, the child's relation with the

mother, and timidity with unfamiliar situations or people showed the poorest predictive success.

As might be expected, allowing Escalona to evaluate the accuracy of her own predictions led to more favorable judgments. When a third psychologist with no knowledge of the original predictions evaluated the preschool behavior, only 45% of the predictive statements were judged correct.

Several major methodological faults make an evaluation of this research difficult. The contamination between prediction and evaluation of outcome could have been avoided. In many cases, groups of predictions were so similarly phrased that if one proved valid, so must the second. Some statements (e.g., "I expect Teddy to value play with peer groups") were the most likely outcomes for any child in this culture.

In general, the authors write somewhat casually in communicating the definitions of complex variables and in specifying the rationale for many of the predictions.

Nor was the reviewer ever sure why the adoption of sex-appropriate behaviors or motor coordination had high predictive validity. Escalona writes that in making the predictions she "relied heavily upon a combination of loosely connected hypotheses, empirically obtained impressions, and imagery of unknown origin." Such a procedure seems to be designed to test the clinical acumen of the predictor rather than the truth value of any theoretical proposition.

Although the authors were emphasizing the developmental significance of infant characteristics, the predictions based upon knowledge of the parents' values and social class turned out to be far more successful than those derived from infant behavior.

Predictive accuracy was not influenced by the age of the infant at the time of the early observations or the age of the child at time of follow-up. This insularity from what are usually regarded as major sources of variance indicates that correctness of prediction was only tangentially related to the hypotheses associated with infant reactivity.

Study of the derivatives of infant be-

W. G. Eliasberg

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havior is a vital developmental problem and the attempt to wrestle with this difficult and complex phenomenon deserves no small measure of praise. The major disappointment is that the strategy was weaker than it had to be.

A Developing Theory of Development

Charlotte Buhler

Der menschliche Lebenslauf als psychologisches Problem. Goettingen: Verlag fuer Psychologie, Dr. C. J. Hogrefe, 1959. Pp. 181. DM 24.-.

Reviewed by HANS A. ILLING

who has his PhD from the University of Berlin as of 1936 but has been comprehending American psychiatry and behavioral science since in graduate work at the University of Utah, Tulane University, and the University of Southern California. He is at present Human Factors Scientist with the System Development Corporation in Santa Monica. He has written many book reviews for many journals.

WHEN the first edition of this book appeared in 1933, it was a completely novel idea to study the life cycle as a whole. The material from which Charlotte Buhler derived her general concepts and her theories at that time was 200 biographies, augmented later by about 1,000 additional biographies. She studied this material in collaboration with about two dozen members of the Psychological Institute of the University of Vienna. The material was destroyed during the Nazi invasion of Austria. Some of Dr. Buhler's concepts, especially that of *Selbstbestimmung* (self-determination), and her theory of the phases of self-determination were discussed later by Gordon Allport in his *Personality* (1938), and in his study *The Use of Personal Documents in Psychological Science* (1942), in which he

chose to apply "self-determination" to individuals of a higher intelligence, whereas Dr. Buhler speaks in general.

Apparently, one of the weaknesses of Dr. Buhler's concept of self-determination, as stated in the first edition of this book, lies in the incompleteness of her definition. The autonomous determination of one's goal in life passes through two phases: the first is experimental and tentative; the other is definitive and specific and is followed by self-appraisal. These terms were purely descriptive; she did not make it clear how this self-determination operates. In the book's second completely revised edition, however, she defines self-determination as the form of anybody's conscious goal-setting that promises fulfillment. Allport's interpretation of self-determination as applying only to superior individuals provided her with the opportunity to clarify this concept. Thus she defines fulfillment as "the results of experiences all through life; above and beyond these successive fulfillments lies the fulfillment of life in toto."

There seems to be no doubt that Charlotte Buhler has derived some of her concepts from Karl Buhler's works, particularly his *Die Krise der Psychologie*, where he dissents from Freud by centering his attention upon the *Kinderspiel*, presenting the dual role of children's play, which, he holds, anticipates the future life cycles. From this theory Charlotte Buhler advances, in her revised edition, to her more comprehensive theoretical frame. This she sets up after her critical survey of the existing concepts and theories of motivation which preceded this book in her *Theoretical Observations on Life's Basic Tendencies*. The author asks herself the question whether there exists some "objective mutuality among the phases of development" which she has set out to present here. She proposes this hypothesis: Childhood constitutes the *Entwurf des Lebens*, the ontogenesis. Wholistically viewed, childhood is an "anticipation and a provisional diagram of life, which is followed in the later years as the *definitive Ausfuehrung* (the definitive realization)." Her new theory, then, is wholistic and interdisciplinary. While derived from biological considerations, the new four "basic tenden-

cies" are conceived as determining the personality on all levels. They are called *need-satisfaction*, *self-limiting adaptation*, *creative expansion*, and *the upholding of the internal order*. Charlotte Buhler, in contrast to Allport, feels that these four basic tendencies are present and varying from individual to individual at all times in the course of personality development.

This view constitutes a change from Freud's *Lehre* as well as from present-day psychology, particularly in America. For instance, Robert A. White applies his concept of life to the middle-age period only, in his *Lives in Progress*. John P. Zubek and P. A. Solberg describe human development mostly in terms of various movements of functions and accomplishments, as does John E. Anderson. There are statistical surveys of biological and psychological data for the course of the life span, like those undertaken by S. L. Pressey, J. E. Ramsey, and R. G. Kuhlen; and there is the group of researchers concerned with the study of biographies, among whom John A. Garrity in his *Nature of Biography* may be cited as representative.

Finally, there is the group of thinkers in comparative theoretical models, particularly Erik H. Erikson. In his *Identity and the Life Cycle* we find a previously published chapter on *Growth and Crises of the Healthy Personality*, in which a model of the main aspect of physical change is given. Buhler refers extensively to Erikson. It appears that the main difference between these two writers is that the latter thinks of his phases as ego development, whereas the former believes she has found concepts with which one can conceptualize the whole of personality maturation.

While Erikson's concepts have become more popular in America and are more acceptable to most psychologists, this bias would not preclude Buhler's concepts from becoming accepted likewise. There are, however, difficulties in the way: First of all, in a space of less than 200 pages, her theoretical discourses are interspersed with a superabundance of biographical material, which actually could fill two volumes instead of a part of one; a separate presentation of the biographical material would, indeed, be in order. Secondly,

although her present concepts are well defined, the reader often encounters ideas with which he is generally unfamiliar. One hears that Dr. Buhler now plans an American edition to demonstrate the application of her new concepts to clinical cases, and that she believes that her concepts can be dealt with in more detail, if, as suggested above, the biographies are treated separately. In this forthcoming book she also plans to relate her theory of human life to psychotherapeutic problems.

Essential and First-Rate

Lee J. Cronbach

Essentials of Psychological Testing. (2nd ed.) New York: Harper & Brothers, 1960. Pp. xxii + 650. \$7.00.

Reviewed by NORMAN FREDERIKSEN

The author, Dr. Cronbach, is Professor of Psychology at the University of Illinois and has been associated with the Bureau of Educational Research there for the last dozen years. Before that he was at the University of Chicago where earlier he took his PhD. He was President of the American Psychological Association in 1957. The review tells more about him. Dr. Frederiksen, the reviewer, after obtaining a PhD at Syracuse University under Floyd Allport, went to Princeton as instructor in social psychology. There his interest in testing developed from joint work with Harold Gulliksen, and presently he found himself on the research staff of the College Entrance Examination Board. He is now Director of Research for the Educational Testing Service in Princeton. With W. B. Schrader he has published a study of the careers of 10,000 veterans in college, *Adjustment to College* (Educ. Testing Service, 1951).

THIS volume first appeared in 1949. It was uniformly praised by reviewers and widely adopted as a textbook. Now a new edition has been pub-

lished which will no doubt be equally well received by teachers of courses in tests and measurement.

Cronbach's primary objective for the revision is the same as for the first edition—to teach the principles of testing in such a way that the student will be able "to choose tests wisely for particular needs, and will be aware of the potentialities and limitations of the tests he chooses." The student who masters this textbook will not only be able to choose tests with discrimination; he will also be well prepared to undertake advanced study of clinical, counseling, industrial, or educational applications of testing.

In the years between the first and second editions of this text, many important advances in the testing field have taken place. The author himself has been responsible for a number of the changes which make a revision desirable. For example, a joint committee of the American Psychological Association, the American Educational Research Committee, and the National Council on Measurements Used in Education worked for five years, under the Chairmanship of Lee J. Cronbach, to develop a set of technical standards for professional practice in the field of testing. The report of this committee was published in 1954 and has been endorsed by the three sponsoring organizations. It has helped to produce a noticeable upgrading of the work of test publishers and in the quality of their text manuals. An important part of the committee's report was a reformulation of the concepts of validity. These contributions, which were anticipated in the earlier edition, are now reflected more fully in the revision.

Through papers in the professional journals the author is largely responsible for attracting attention to an important aspect of test-taking behavior which is commonly known as *response set*. An example of a response set would be a consistent tendency on the part of an examinee to choose the *true* or *agree* option, or to choose the response he considers to be socially desirable. Failure to take account of response sets in developing certain kinds of instruments, particularly personality inventories, may lead to serious errors in interpreting

scores. The pioneering work of Cronbach in this area has stimulated many other investigators to take a critical look at some of our best-known self-report scales, with the result that much of the work on such instruments as the F-scale and the MMPI must be re-examined. Cronbach includes in his revision material reporting on this area of activity. The use of verification and correction keys for control of faking and response sets he presents clearly.

Other developments in the testing field which are reflected in the edition are the development and validation of differential aptitude test batteries, studies of the effects of coaching (a particularly timely topic, with pressures on college admissions officers mounting), comparisons of clinical and actuarial interpretations of test scores, and performance tests of personality.

Performance tests of personality usually involve the notion that the subject does not realize what personal characteristics are being measured by the tests. He may take a test which he believes is an inventory of opinions but which is really scored to reveal some other personality characteristic. Ethical problems are involved in such approaches, particularly if the scores are reported to others for use in selection or placement. Cronbach frankly faces ethical problems of this sort in the light of the American Psychological Association's *Ethical Standards for Psychologists* and of such critical comments as those in *The Organization Man*.

LIKE the earlier edition, the new book is readable and teachable. Features of the earlier book which are preserved are the Computing Guides (which provide simple computational models for such things as product-moment correlation and standard scores) and student questions. These questions are not relegated to the ends of chapters but appear in black-face type in the text at appropriate points. The questions require the student to practice doing something he has just been taught ("Using Figure 15, interpret each of the following in percentile terms: a *z* score of 3.0; . . . a *T* score 65") or to think originally about a problem ("How might lack of self-confidence help one student

to attain high marks, yet be a drawback to another?").

The book is not merely an excellent textbook for the advanced undergraduate or the beginning graduate student. Many psychologists will find it a useful reference work, a work which is adequately indexed and rather thoroughly documented with 572 references. The volume is not a catalog of published tests, although it describes a large number of tests which are widely used or which exemplify certain types of tests. Neither is it a treatise in statistics or test theory. It is exactly what its title implies: a presentation of the essentials of psychological testing. It should provide a very solid foundation for more specialized training in special fields.

Missionaries among the Dyads

Henry L. Lennard and Arnold
Bernstein

*The Anatomy of Psychotherapy:
Systems of Communication and
Expectation.* New York: Colum-
bia University Press, 1960. Pp. xx
+ 209. \$6.00.

Reviewed by ROSALIND DYMOND
CARTWRIGHT

Both the authors are Columbia PhDs of the early 1950s. Dr. Lennard is a Project Director of the Bureau of Applied Social Research at Columbia and Visiting Professor of Sociology at the New School for Social Research. Dr. Bernstein, a psychoanalytic therapist, is Assistant Professor of Psychology at Queens College and Chief of the Psychological Clinic at Stuyvesant Polytechnic Hospital. Dr. Cartwright, the reviewer, was trained up to the MA in clinical psychology at the University of Toronto and up to the PhD in social psychology at Cornell in its Department of Sociology and Anthropology. For the past nine years she has been involved in research in psychotherapy at the University of Chicago. With Carl Rogers she was a co-editor of *Psychotherapy and Personality Change* (Univ. Chicago Press, 1954).

IN this book a sociologist, Lennard, and a psychoanalytic psychotherapist, Bernstein, report the results of their intensive study of eight cases of psychotherapy. In terms of the old distinction in this field between studies of process and outcome, this is a process study. The first eight months of the interviews for each of two patients of the four participating psychoanalytic psychotherapists was fully tape-recorded. In addition patients and therapists responded to questionnaires and interviews designed to reveal their role expectations, values, and evaluations of the interviews at intervals during the course of the therapy.

The authors state the purpose of the research to be "to apply concepts and methods developed within the framework of the social sciences—especially concepts pertaining to the study of face-to-face interaction and methods pertaining to the analysis of communication—to the study of psychotherapy." They acknowledge their indebtedness to the research and ideas of Bales, Merton, Parsons, Bateson, Bertalanffy, and Speigel. They go on to emphasize the newness of this venture by saying that "social science concepts have not hitherto been applied to psychotherapy in any systematic fashion or with any major investment of effort," and that one of the intentions of the book is "to show that the approach and concepts of one discipline (sociology) could contribute to an understanding of problems faced by another discipline (psychiatry)."

These statements led this reader to anticipate that the natives, floundering in a state of ignorance, were to be brought into a state of scientific enlightenment or at least introduced to new ways of viewing psychotherapeutic interaction, new concepts for organizing and understanding the systematic patterns, and new methods for dispelling the magical beliefs and for revealing the truth about what goes on in psychotherapy. The bulk of the book reports patterns of verbal interaction and changes in them during the first eight months of the therapy for these eight cases. There are not many surprises, but one is that there is so little that is new in the results. Of course the mis-

sionaries did not first enquire into the state of sophistication of the group they were out to educate. Their bibliography contains references to only two research papers on psychotherapy, whereas there are in the literature many studies directly relevant to this undertaking.

The second surprise is the crudeness of their handling of the data. Mostly data are reported as tables of proportions of statements falling into various categories. Tests of significance were applied in only two instances and then it is difficult to tell from the report whether or not they have been applied correctly.

THE meat of the book for this reader lay in the comparison of the two more active therapists to the two less active ones in terms of the differences in their patients' behaviors. Dyads in which therapists were more active showed fewer signs of strain (broken appointments, discontinuance of therapy, patients' complaints). Active therapists did more role-teaching early in therapy than did the passive therapists and produced more statements in this category than did their patients. Passive therapists, on the other hand, produced fewer statements concerning their roles and interactions than their patients early in the therapy. Later in therapy the patients of the more passive therapists show a rise in the number of statements about their role and that of the therapist, a difference which the authors take as a sign of disequilibrium in the system. Patients rated twenty-three of their interviews as proceeding "more easily," "about the same," or "less easily" than preceding ones. From comparisons of these it seemed clear that patients preferred sessions in which the therapist is more active, in which his activity is highly structured and in which the proportion of therapist statements referring to affect is high. These characteristics they interpret as giving the patient a feeling of being understood. However, the extent to which such results may be generalized is greatly limited by the size and unrepresentativeness of the sample both of patients and therapists.

In terms of the total contribution of this book then, we may ask: Was this

trip really necessary? It is this reader's opinion that it would have been more important in 1940 than in 1960. At about that time and for the following ten years or so, many single cases and small samples of cases were studied intensively to discover the patterns of the way therapy proceeded: the differences between what went on in the early phases versus the later phases, for example. Some of these were even so 'social-science' oriented as to apply Chap-ple's interaction chronograph to complete cases; some studied differences in patients' verbal behavior when the therapist took a leading versus a following role; another, the kind and extent of patient response following a 'reflection' versus an 'interpretation' by the therapist. This book lies in this general tradition and adds its bit that therapists who educate their patients in terms of the role behavior expected of them, keep them as patients longer and happier, and also that patients are more satisfied with sessions where the therapist is more active than with those in which he is less active. Whether either of these relations is a 'good' thing in terms of achieving goals for the particular patient is outside the scope of this study.

The authors say in summary that it is their opinion that "the most important contribution of therapy lies in the experience in the total and recurrent pattern of patient and therapist interaction extending over an enduring period. The enormous amount of similarity in the therapeutic systems suggests to us that what is shared by different therapist-patient pairs may be at least as therapeutic as that which is unique." And then again it may not be, for there is no way of telling from this type of study. To put first things first, the answer to the question *Is psychotherapy therapeutic?* is still unknown. This answer waits on a properly established criterion measure or a series of them. Surely here is the place where research in psychotherapy can use all the help it can get. The type of study reported here does more to add to the natural history of psychotherapy than to push forward the boundaries of knowledge.



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This introductory text now includes three valuable chapters on "Psychological Measurement," "Social Behavior," and "Attitudes, Values, and Opinions," the latter two written by Professor Hubert Bonner, of Ohio Wesleyan University. The text has been further expanded by the addition of new case references and other illustrative material and by more comprehensive discussions.

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More about Drugs and Psychology

Robert M. Featherstone and Alexander Simon (Eds.)

A Pharmacologic Approach to the Study of the Mind. Springfield, Ill.:

Charles C Thomas, 1959. Pp. xviii + 399. \$10.75.

Reviewed by ROGER T. KELLEHER

The editors are both at the University of California's School of Medicine. Dr. Featherstone as Professor of Pharmacology and Dr. Simon as Professor of Psychiatry. Dr. Kelleher, the reviewer, is Senior Pharmacologist of the Smith Kline and French Laboratories in Philadelphia. He was for a while at the Yerkes Laboratories in Orange Park, Florida, studying conditioned reinforcement and concept formation in chimpanzees, but for the past three years has been working with the pharmacologists on the behavioral effects of various drugs.

MULTIDISCIPLINARY symposia on drugs that affect behavior are enjoying a current vogue. This book contains 44 reports and short communications written by the psychiatrists, pharmacologists, biochemists, and psychologists who participated in a three-day symposium at the University of California's San Francisco Medical Center in January 1959. The purpose of the book is to "bring into critical focus the questions which must be answered before the so-called 'mental diseases' can be approached successfully with the use of chemical compounds."

The subject matter presented ranges from *A Clinical Approach to Mental Diseases* (W. Malamud) to *A Review of the Effects of Monamine Oxidase Inhibitors on the Organs of Digestion* (D. Liebowitz). The inclusion of the latter topic in a book entitled *A Pharmacologic Approach to the Study of the Mind* will appeal to some who believe that the mind is as likely to be found in the gastrointestinal tract as elsewhere. Many of the participating scientists did indeed object to the symposium's title on the grounds that the

concept of mind is linked with an outmoded dualistic philosophy.

The editors have divided the book into three sections that correspond to the program on each day of the symposium. The first contains four reports: S. S. Kety describes the effects of drugs upon cerebral metabolism, S. Udenfriend discusses biochemical factors in phenylpyruvic oligophrenia, J. Elkes criticizes conceptual models in psychopharmacology, and W. Malamud analyzes the clinical advantages and disadvantages of drugs that affect behavior.

The second section opens with a panel on *Non-Empiric Approaches from the Basic Sciences*, with H. Hunt representing psychology. Hunt devotes most of his discussion to a survey of the "operant approach" to pharmacology; however, he expresses misgivings about the growing popularity of the present instrumentation and the emphasis on empiricism. He commits the technical error of noting drug dosages without specifying the route of administration. In a subsequent panel on *Problems of Research Design and Clinical Evaluation*, J. A. Starkweather briefly describes an interesting experimental demonstration that the effects of sodium phenobarbital or *d*-amphetamine on human subjects can be reversed by social interactions with other drugged subjects. Following the panel discussions are three reports on *Depressants (Tranquilizers)*.

The third section opens with a panel on hallucinogenic drugs. A. Holliday, the psychologist on this panel, discusses the unscientific methodology that characterizes many psychological studies of hallucinogens. Holliday also presents an amusing account of the way in which a tentative conclusion that is repeatedly stated can attain the status of a fact in the psychological literature. The remain-

der of the third section consists of reports on the pharmacology, toxicology, biochemistry, and clinical effectiveness of drugs which alter brain chemistry by inhibiting the enzyme, monoamine oxidase. Some monoamine oxidase inhibitors are reported to be effective in the treatment of psychotic depression. The participants emphasized, however, the view that extensive research will still be required to determine the extent of any correlation between enzyme inhibition and clinical efficacy.

THE book also includes an interesting dinner speech by Aldous Huxley. Huxley is alarmed at the rapid development of efficient psychological and pharmacological techniques for controlling human behavior. He argues that individual liberties have been preserved because governments employ inefficient techniques of control. He notes, however, that stopping technological advances is out of the question. Although he makes no specific suggestions for escaping this dilemma, a statement near the beginning of his talk suggests that there may be no dilemma. In discussing the fundamental rules of morality, he remarks: "We see then that many of the most important ethical truths flow quite naturally and simply from the scientific facts. . . ." The course of events in the past 25 years has shown that many of Huxley's statements are prophetic.

Although many of these reports will interest psychologists, the book can not be recommended without certain qualifications. First, the book is poorly organized; the diverse subject matter in each section is not integrated by unifying themes. Secondly, the book does not present a balanced survey of drugs that affect behavior; for example, widely used drugs such as chlorpromazine and meprobamate receive little attention. Thirdly, much of the ground that is covered has been more thoroughly covered in other published symposia. Finally, the quality of the book's binding is poor, and the six pages of photographs of the participants add nothing.



Only what society values will finally be done well.

—THOMAS GRIFFITH

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535 pp., illus., \$6.00

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What Makes Men Antisocial?

Benigno Di Tullio

Principi di criminologia clinica e psichiatria forense. (2nd ed.) Rome (Piazza Della Liberta 20): Istituto di Medicina Sociale, 1960. Pp. xii + 510. L. 3.000.

Reviewed by LUIGI TAGLIACCOZZO

Dr. Tagliacozzo has, from the University of Rome, PhDs in both law and education and for a dozen years occupied himself with teaching education, philosophy, and psychology in Rome and in administering instruction. Granted fellowships by the Institute of International Education and the U. S. Public Health Service, he specialized in psychotherapy and has for the past dozen years been giving counseling and guidance in Chicago. He is now engaged in psychotherapeutic private practice in Chicago. He still finds his early training in philosophy valuable and is at present busying himself with the ego-psychology of Paul Federn and Eduardo Weiss.

THE Italian school of criminology has been known since the work of Lombroso and Ferri for its interest in the so-called "delinquent constitution," which can be defined as the combination of physical and psychological characteristics that, under specific environmental influences, may lead to antisocial and criminal behavior. Professor Di Tullio, who has been a student of Ottolenghi (who was in turn a disciple of Lombroso), is today one of the most authoritative representatives of this school. His influence—both as a scholar and as a teacher—has also made itself manifest in the Italian penal policies and practices as well as in the fields of prevention and rehabilitation.

The book under examination should be primarily viewed as a university textbook and a manual for advanced students who wish to specialize in the field of criminology and related clinical problems. As such it tends to be mostly informative, even though it presupposes in the reader a considerable amount of

biological, medical, and psychological knowledge.

The author defines his discipline as "the science of human antisocial and criminal behavior, based upon the study and deep analysis of individual cases both normal and abnormal or pathological." Crime is a biosocial phenomenon and its understanding presupposes the study of the biological foundations of the personality as well as of environmental factors. According to the author's fundamental assumption it is only the "meeting of the two vulnerabilities, a personal internal one and a social external one, that can give origin to a specific criminal action." This point of view implies the acceptance of a plurality of causes for antisocial phenomena and, therefore, a multidisciplinary approach to the study of criminology. In the author's words, "clinical criminology will accept and utilize the contributions of physical and social anthropology, of sociology, of neuropsychiatry and psychology without adherence to any particular theory or school."

A THOROUGH discussion of the implications of such a theoretical position is obviously beyond the limits of this review (see instead E. H. Sutherland and D. R. Cressey: *Principles of Criminology*, Lippincott, 1955, 59-62). The reviewer, however, feels the need to question whether the book does justice to the author's fundamental assumption; he also wonders whether—in spite of the author's intention—in the course of the analysis each factor (constitution and environment) has become an abstract and static entity, with a consequent loss of the concrete meaning of the process of interaction. From these points of

view the book lends itself to several criticisms. The author is undoubtedly well acquainted with contemporary theories and discoveries in the disciplines mentioned above, matters which he considers indispensable to the study of clinical criminology. He appears to move easily from the typologies of Pende, Kretschmer, Sheldon, and Verdun and the biological studies of A. Carrel to the personality studies of Father A. Gemelli or to the depth psychologies of Freud, Jung, and Adler, from the descriptive psychiatry of Kraepelin to the dynamic conceptions of Alexander or Jasper, from the child psychiatry of De Sanctis to Aichorn and Bettelheim and from Hooton or the Gluecks to Sutherland and Barnes. This eclectic approach would appear to be detrimental to an integrated appraisal of the phenomena under consideration, if not at times even confusing.

It can easily be conceded that a book of 'principles' can only present the results of scientific research rather than research itself. It is unfortunate, however, that often theories and conclusions are reported in this volume without sufficient validating evidence or adequate mention of the criticism that subsequent research has developed in relation to the conclusions in question. This deficiency is particularly evident whenever constitutionalistic theories are presented.

It appears to this reviewer that, in spite of his effort toward an integrated approach, Professor Di Tullio's emphasis remains often mainly on biological factors: as a physician long associated with the works of the Italian endocrinologist Nicola Pende, he dedicates large portions of his book to the study of the relationship between heredity, endocrine dysfunctions, organic brain pathology, feeble-mindedness, tuberculosis, syphilis, epilepsy, and crime. To be sure, none of these conditions is considered a 'cause' of crime per se, but only a predisposing agent. The fact remains, however, that reference to the numerous studies which seem to disprove the significance of the relationship between these factors and crime is too often insufficient.

It is perhaps as a consequence of the author's predominantly medical back-

ground that the sociological point of view receives in the book less attention. This more limited interest in sociological theories is acknowledged by the author himself (p. 55). Such a unilateral tendency, however, cannot fail to handicap the understanding of the phenomenon of crime, particularly since the phenomenon is not clearly defined in the book within a sociological frame of reference. In effect, the author seems to speak at times of crime and antisocial behavior in ways that do not appear free from naturalistic biases, as abstract data not defined in terms of the social context in which they develop. Thus, the author shows a positive disinterest in two of the most characteristic phenomena of modern crime: the white-collar crime and the organized crime. It might very well be that neglect occurs because these two phenomena are less prominent in a less industrialized and urbanized country like Italy, but this bias again would only confirm the shortcomings of a predominantly constitutional, biopsychological approach.

In summary, the present reviewer, placing himself in the position of a neophyte in the field of criminology, comments that he found in this book a great wealth of information on criminological hypotheses and theories, but that he could not avoid a sense of frustration. He felt himself confronted with an array of different tools designed to gain an understanding of crime and of the individual criminal, without his being able to find an adequate analysis of their relative values or integrated use. Particularly the chapters on *Criminogenesis* and *Criminodynamics* appeared—in spite of their titles—lacking in this regard and also regrettably poor in respect of case histories and clinical presentations.



Our mind is so fortunately equipped, that it brings us the most important bases for our thoughts without our having the least knowledge of this work of elaboration. Only the results of it become conscious. This unconscious mind is for us like an unknown being who creates and produces for us, and finally throws the ripe fruits in our lap.

—WILHELM WUNDT (1832–1920)

Mysterious Eve

Theodor Reik

The Creation of Woman. New York: George Braziller, 1960. Pp. 159. \$3.75.

Reviewed by RUTH E. HARTLEY

The author, Dr. Reik, is the well-known psychoanalyst, who studied with Freud, has lectured at Vienna, Berlin, and the Hague, and has written nearly a score of books in German and a dozen in English, of which some of the more recent are Listening with the Third Ear, 1948, The Secret Self, 1952, Myth and Guilt, 1956, and Mystery on the Mountain, 1959. Just now he is president of the National Psychological Association for Psychoanalysis. The reviewer, Dr. Hartley, is a visiting professor at the College of the City of New York and is directing research on the development of children's ideas about women's and men's social roles. She is known for writing with E. L. Hartley, her husband, Fundamentals of Social Psychology, and she is also the joint author of two books about children's play.

THE third in a trilogy of works dealing with the prehistorical bases of the Old Testament, this book, by the author's testimony, had a gestation period of forty years. (*Myth and Guilt*, 1956, and *Mystery on the Mountain*, 1959, are the first two volumes of the trilogy.) Using a method he calls "archeological psychoanalysis," Dr. Reik here attempts to resolve the apparent conflict inherent in the two accounts of the creation of Eve as contained in Genesis, by treating them as greatly telescoped versions of folk myths originating at vastly separated periods in time.

The author is concerned mainly with the second version of the tale, which depicts Eve as being created from Adam's rib. He concludes, after considerations involving puberty rites of preliterate groups and the religious traditions and myths of Mediterranean and Near-Eastern peoples, that the Eve story is, in reality, the disguised ac-

count of the puberty rites of the ancient nomadic tribes who furnished the background for the early Biblical tales. Dr. Reik believes that the removal of Adam's rib stands for some other mutilation, probably circumcision, and that the creation of Eve from the rib is an example of myth-making "dream-work." The latter represents the idea that the mutilation led to the acquisition of a mate by the condensed statement that it 'became' that mate.

The above interpretation is, however, only one of the meanings of the Genesis tale. The author also concludes that it was designed as a hoax on the uninitiated (apparently, the women and younger children of the tribe), and that it conceals an earlier religious tradition involving an Earth Goddess and her incestuous relations with a son, a tradition which might have been suppressed by Moses.

Dr. Reik employs a method of reasoning which seems to depend on the transformation of analogy into identity. For the reader who accepts the validity of conclusion by analogy, this book may be highly enlightening. Even for Philistines who insist on more tangible evidence, it can be good fun as an exercise in scholarship and ingenuity. Much depends on the intensity of the reader's concern with the book's central theme.

This reviewer must confess to finding Dr. Reik's fondness for spiral reasoning tedious at times. His repeated return to points that had apparently been disposed of earlier added nothing to the clarity of his exposition. In many ways his style is reminiscent of the method of free association. While this may not be surprising in a psychoanalyst of such long standing as is Dr. Reik, it may nevertheless present hazards for the general reader.

The first part of the book offers an impressive survey of the interpretations of the Eve story by Biblical scholars, poets, and philosophers. In the second part, a wide-ranging account of the puberty rites of many peoples is presented. Both bodies of information are interesting in themselves. The juxtaposition of these bits and pieces, however, makes them even more interesting, if one wishes to join the game of interpretation. Scholars and philosophers come

out as blatant misogynists, showing a vitriolic hostility toward the female sex, matched only by the utterances of ten-year-old schoolboys. Considering the cited puberty rites as evidence, one might conclude that this concentrated

bitterness arises from a species of reverse penis-envy. If Dr. Reik's conclusions are correct, he has given us a glimpse into the ancient history of the war of the sexes, a conflict which never seems to lack for enthusiastic volunteers.

Anthology of Logical Positivism

Alfred J. Ayer (Ed.)

Logical Positivism. Glencoe, Ill.: Free Press, 1959. Pp. viii + 455. \$6.75.

Reviewed by HERBERT FEIGL

The author, Dr. Ayer, is now Professor of Philosophy at Oxford University. For many years he was Grote Professor of Mind and Logic at the University of London. He has taught at New York University and lectured at many universities from China to Peru. He is one of the hundred members of the British Academy. His principal books are *Language, Truth and Logic* (1936), *The Foundations of Empirical Knowledge* (1940), *Philosophical Essays* (1954), and *The Problem of Knowledge* (1956). The reviewer, Dr. Feigl, is Professor of Philosophy at the University of Minnesota and, since its foundation in 1953, Director of the Minnesota Center for Philosophy of Science. He is an Austrian with a doctorate magna cum laude from the University of Vienna. At the age of nineteen he won a prize for an essay on the philosophical significance of Einstein's theory of relativity. He was an active participant in the Vienna Circle where logical positivism was born and he became this movement's first missionary to America, working first at Harvard with Bridgman and the Harvard logicians, and then later at Iowa until he went to Minnesota in 1941. He has been visiting professor at the Universities of Columbia, California, and Hawaii and co-editor with W. Sellars of *Readings in Philosophical Analysis* (Appleton-Century-Crofts, 1949) and with M. Brodbeck of *Readings in the Philosophy of Science* (Appleton-Century-Crofts, 1953).

IN this second volume of the Library of Philosophical Movements, A. J. Ayer has assembled some of the more important and influential papers of the

logical positivists and a few closely related papers from the British movement of philosophical analysis. The essays provide a fair survey of the major tenets, developments, and controversies within these schools of thought. One reads (or rereads) the earlier papers by the members of the Vienna Circle with a certain nostalgia. The issues were simple and clear-cut; the doctrines had a strong "prohibitionist" flavor. Occam's razor, prominently in the form of the empiricist meaning criterion, aroused the anxiety as to whether what one wanted to say made factual sense. The elimination (*Ueberwindung*) of metaphysics was forcefully advocated. But things are not so simple any more. This fact comes out persuasively in the last essay of the book, written by the late F. Waismann. He still says that many philosophical problems need dissolving rather than solving; yet he does not wish to restrict the task of philosophy in the clarification of ideas. In fact, he speaks of a "clarity neurosis" which afflicts most positivists and linguistic analysts.

What is the value of this book for psychologists? Since most issues dealt with are basic philosophical ones (the methodological essays of the positivists are not here included), the interest of the larger part of the present volume is predominantly academic and relatively remote in its bearing on the foundational questions of psychology. There is, however, one notable exception: Carnap's essay on *Psychology in Physical Language*, originally published in 1931 (and here presented for the first time in English translation), will remain one of the classics of logical behaviorism. It is worth noting that Carnap (whose

inspirations came from J. B. Watson, B. Russell, and O. Neurath) anticipates many of the arguments of B. F. Skinner as well as of Gilbert Ryle. The idea of psychology as part of a unified intersubjective science is here developed with incisive insight and cogent reasons. Some modifications along the lines of more recent developments in his outlook are indicated in a brief appended note (of 1957) by Carnap himself. Much that Carnap has to say on our knowledge of other minds, or intuition and empathy, on introspection, and especially on the acquisition of subjective terms in the learning process, will remain of enduring value.

The book contains also the (often reprinted) article by C. G. Hempel on *The Empiricist Meaning Criterion*. This is also supplemented by the author's remarks (of 1958) on his more recent position in these highly intricate and controversial matters. The reader will see that a full logical explication of the confirmability criterion is a delicate and difficult task.

The book begins with a helpful introduction by A. J. Ayer in which the vicissitudes in the development of logical positivism and of linguistic analysis are helpfully sketched. This will provide for many readers a much-needed general orientation. The first selection is Russell's prophetic essay on *Logical Atomism* (this is not his longer paper of the same title, but the briefer version first published in J. H. Muirhead, ed. *Contemporary British Philosophy*, 1924). There are many other gems of lucidity among the seventeen papers in this collection. Hans Hahn's *Logic, Mathematics and Knowledge of Nature*, for example, discusses with admirable clarity the basic difference between the purely formal and the factual-empirical sciences.

As Ayer himself explains, he has "tried to illustrate the historical development of logical positivism, the range of its interests and the main points of controversy. Lack of space obliged me to pass over many pieces that I should have liked to include." This reviewer agrees that, on the whole, the selections were made wisely, and that the outcome is a book that will retain interest for some time to come.

What Bends the Twig?

Daniel R. Miller and Guy E. Swanson

Inner Conflict and Defense. New York: Henry Holt, 1960. Pp. x + 452.
\$6.95.

Reviewed by ELSA E. ROBINSON

The authors-editors-directors of this study are both at the University of Michigan: Miller a psychologist, and Swanson a sociologist. They are the authors of an earlier related study, The Changing American Parent (Wiley, 1958; CP, Oct. 1959, 4, 318f.). Miller hails from Stanford and Swanson from Chicago, each with a 1948 PhD. The reviewer, Dr. Robinson, is Associate Professor of Psychology at New York University, where she took her PhD twenty years ago. She has been associated with the Judge Baker Foundation in Boston and the Bellevue Medical College in New York and is especially interested in the psychology of development in infancy and early childhood.

NO one has ever seriously denied that the way in which a child is reared affects the development of his personality. Nor can there be any doubt that the customs of particular social groups affect the ways of members of those groups, including the way in which they bring up their children. If *a* is a function of *b* and *b* of *c*, then *a* is of *c*. It is a case of this proposition that most of the researches included in *Inner Conflict and Defense* are designed to explore.

The investigations reported are the work of a group of six psychologists and sociologists participating in seminars at the University of Michigan under the direction of Professors Miller and Swanson, psychologist and sociologist, respectively, who provided the theoretical basis for the studies and edited and coordinated the whole.

The groups studied are distinguished by their membership in the 'middle' or 'working' class and their identification with 'entrepreneurial' or 'bureaucratic'

integrations. The latter distinction is the focal issue in an earlier study by the same authors, *The Changing American Parent* (Wiley, 1958), reviewed in *CP* (Oct. 1959, 4, 318f.). An oversimplified definition would describe the 'entrepreneur' as the risk-taking man who is self-employed or a member of a small business, farm, or industry, and the 'bureaucrat' as the secure, specialized member of a large organization. As the authors agree, neither this distinction nor that between 'middle' and 'working' class is sharp, and some of the other criteria seem blunt tools indeed, except perhaps in a statistical sense: a family is classed as entrepreneurial, for example, if either parent was born on a farm or born outside the United States, and a laborer may become a member of the middle class by virtue of an unusual amount of education.

With these social differences the authors associate differences in home atmosphere generally, as well as in a variety of rearing practices, of which the principal ones are age and manner of weaning and of toilet training, kinds of parental discipline, frequency and nature of rewards for good behavior, attitudes and practices regarding obedience to parental commands. All this information was derived during one interview with each of the mothers of the subjects, 232 boys in the seventh, eighth, and ninth grades of the public schools in Detroit. Although the authors defend the accuracy of the interview data, one wonders how many anxious and self-conscious mothers of our era, loaded with the child-rearing precepts that stream from the mass media, can be wholly honest about their methods of discipline and the like. As for the dating of weaning and toileting, it has been demonstrated by Pyles, Stolz, and Mac-

farlane (1935) and by McGraw and Molloy (1941) that maternal recollections of events in infancy are quite unreliable, even when the happening recalled is much more recent than the 10 to 12 years of the present study.

THE investigations attempt to relate differences of social class (including integration) and of methods of child rearing with indices of the personality of the subjects, specifically, their characteristic defenses under conflict. An over-all hypothesis derives from reports that schizophrenics and manic-depressives tend to come from lower-class and middle-class areas, respectively. The authors predict the same direction of difference in the defensive behavior of normal members of the two classes.

The formulation of the experimental hypotheses depends upon assumed or ascertained differences among the social groups in their values and aspirations, the kind of model they present to their offspring, and their child-rearing practices and attitudes. From this base the authors proceed to speculate about the probable repercussions of these differences on the development of personality. These conjectures beget the specific hypotheses which are then put to the test. Their number is formidable. Among them are such expectations as that working-class children will be more directly aggressive than those of the middle class; that the harsher discipline and scantier gratifications provided working-class boys will make them more prone to deny failure by fantasy withdrawal; that early and coercive toilet training will lead to more severe feelings of guilt.

The tests used are for the most part projective, with heavy emphasis on the story-completion technique. The stories are ingeniously calculated to serve the purposes of each particular study. The situations are natural and interesting. The protagonist is always a boy with whom, it is assumed, the subject identifies himself. The identification need not occur, of course. About "a few subjects" whose story endings revealed no defenses at all the authors conjecture that "their omission itself represents some kind of defense." Since these tests are the sole basis for judging the cru-

cial variable, the subjects' defenses, it is unfortunate that "because of the pressure of time" it was possible to validate only one of the many instruments against an independent criterion.

In view of the number, complexity, and vagueness of the independent variables, especially those of social class and integration, it is not surprising that many of the specific hypotheses are not confirmed by the results of the studies, although a good number are. Differences in personality defenses between 'bureaucratic' members of the two classes are negligibly small. But it appears that children of 'entrepreneurs' of the middle class are marked by the intense parental pressures to which they are subjected.

Inevitably in such phenotypical research the face of the individual child is lost. It is nevertheless important to recognize the mounting evidence that no necessary relationship exists between specific parental practices and their effectiveness, that the behavior outcomes of those practices appear to depend at least in part on what some have called the primary reaction patterns evident by the age of three months.

The study is a bold one, and its authors are properly aware of the difficulties and uncertainties which beset the task they have attempted. In addition to some positive experimental results, the primary value of their work lies in the acuteness and perspicuity of much of the theoretical discussion and in the ingenuity of the techniques employed.

Margaret Mead comments in her introduction to this work, both sexes are offered exploitative roles, "the boy playing on each girl's hope of permanence and early marriage to get cheap and immediate and unsatisfactory sexual compliance, the girl playing on the vulnerability of the boy who begins to court her by entangling him in a sexual intimacy which will propel him toward marriage."

To provide a more rigorous description of this situation, Ehrmann adopts a six-fold classification of primary stages of heterosexual behavior, from holding hands (or less) to coitus. This is used to specify the most advanced behavior the individual has ever experienced, the most advanced behavior in the current month, and the behavior characterizing the typical date. On the basis of questionnaire responses from 841 college students and interviews with 100 of them, these figures are related to frequency of dating, number of dating partners, control factors, comparative social class of dating companion, going steady and being in love, attitudes toward sex, happiness in general, source and adequacy of sex instruction, age at puberty and at first date, church attendance, home discipline, and parents' occupation. Of the innumerable permutations of the data, only those are summarized in the 40-odd tables that seem relevant for a few principal themes.

THE real contribution of the survey is not found in such information as the proportion of nonvirgins and the variety of coital partners. Indeed, these are best left unmentioned lest, like the overenthusiastic designer of the book's jacket, one accept them as representative of "the sex behavior of modern American youth." All they represent, as the author points out, is the population of several postwar classes in Marriage and the Family, in a college where coeds were outnumbered eight to one. More significant, perhaps in terms of generality and certainly as a demonstration of a fruitful methodology, are the more involved findings that point to the existence of two quite different subcultures. (1) There is greater correspondence in these females than in these males between most advanced

Love, Sex, and Dating Data

Winston Ehrmann

Premarital Dating Behavior. (Introduction by Margaret Mead.) New York: Henry Holt, 1959. Pp. xx + 316. \$6.00.

Reviewed by FRANK W. FINGER

Dr. Ehrmann, the author, is Professor of Sociology in Colorado State University. He was formerly director of the Marriage and Family Clinic of the University of Florida, and that experience plus five years' service in China, Burma, and India where courtship involves no highly personalized dating accounts for his interest in this topic. Dr. Finger, the reviewer, is Professor of Psychology at the University of Virginia. He is a rodent psychologist, trained at Brown University by Walter Hunter and Harold Schlosberg before the Second World War, and has been at Virginia ever since the war. His main concern at present is the psychology of motivation.

THE popular interest in the Kinsey reports was based on their revelation in stark numerical terms of widespread deviation from standards understood to be part and parcel of American life. The present report will attract more limited attention, except in the popula-

tion studied, because it represents a more intricate and pedestrian task. It was natural for the zoologist to emphasize the biological aspect of sex, the 'outlet' or orgasm; it is not surprising that sociologist Ehrmann would attempt to delineate the pattern of associated attitudes, social characteristics, and modes of control against which the whole gamut of sexual behavior can more meaningfully be considered.

A number of forces in our society combine to make the period between puberty and marriage a stressful one. The romantic aspects of love and marriage surround the girls, erotic stimuli continuously bombard the boys, pairing off is encouraged but sexual gratification prohibited—and the adolescent is cast loose with little informed advice from his elders. The complex rituals subsumed under the term *dating* reflect the struggle to resolve the "conflicting ideas of sex and love and of what constitutes maleness and femaleness." As

stage of sex ever experienced and current behavior. (2) Nonvirgin females report a higher frequency of coitus than do nonvirgin males. (3) In dating, and especially in experiencing intercourse, males cross social class lines downward much more frequently than do females. (4) Attitudes toward sex are more closely related in the male to the stage of behavior, in the female to the frequency and initiation of the most advanced stage experienced. (5) There is virtual coincidence between sex behavior and the avowed code of personal conduct in females, while the male is often unable to go as far as his code would permit. (6) The male who dates only 'a steady' is less likely to practice coitus than one who does not, but the converse is true in the female. (7) Being in love increases for the girl the practice and acceptability of premarital intercourse, but decreases them for the boy: "Love tends to equate the sexual expression of males and females."

The reader, either of the book or of this review, will need to exercise caution in the interpretation of the data out of the procedural and statistical context. The total *N* of the interview sample is not large, and sometimes after successive subdivisions the groups yield merely suggestive trends. The level of test-retest reliability leaves much to be desired (reported coefficients range down to .44, and are considerably lower than some in the literature). The chief indication of validity is the interviewer's impression of his students' sincerity, and at several points he is forced to question the accuracy of their perception and recollection.

But these shortcomings are only relative. More important is the guidance furnished for research yet to be done. It has been clearly demonstrated that the understanding of sexual behavior is incomplete if just its biological properties are examined. The resolution of the 'dilemma of youth' depends upon the extension of such comprehensive approaches as Ehrmann's to more diverse and adequate population samples.



The Final Diminution of Drive?

Marshall R. Jones (Ed.)

Nebraska Symposium on Motivation. Vols. VI and VII. Lincoln: University of Nebraska Press, 1958, 1959. Pp. x + 278; x + 243. \$3.00 each volume.

Reviewed by JOHN W. ATKINSON

Dr. Atkinson, the reviewer, is Professor of Psychology at the University of Michigan. He became interested in the problem of motivation fifteen years ago, stimulated in part by McClelland when Atkinson was at Wesleyan. He has been getting more deeply involved ever since. He is a joint author, with McClelland, Clark, and Lowell, of The Achievement Motive (Appleton-Century-Crofts, 1953). He was editor of Motives in Fantasy, Action, and Society (Van Nostrand, 1958) and a contributor to it. This year he holds a Guggenheim Fellowship during which he hopes to complete his book on theories of motivation.

Now in its eighth year, the *Nebraska Symposium on Motivation*, edited by Marshall R. Jones, continues to satisfy an important need by keeping attention focused on a field in a state of conceptual upheaval. Each of the annual volumes contains six papers and comments by invited participants actively engaged in research on some motivational problem. This review will consider a few major themes, rather than the more obvious diversity of content, which characterizes the whole set, with special attention to the two most recent volumes. (See also *CP*, Aug. 1956, 1, 229f. on Vol. III.) The 1959 volume is the first to appear in letterpress edition and to include an index.

The stylistic freedom given participants is a mixed blessing. It affords opportunity for expansive programmatic comment and instructive argument concerning the tactics and strategy of research as varied as neurophysiological study, factor analysis, and cross-cultural study; but it also encourages irrele-

vant longwindedness which, on occasion, blunts the point of an argument.

In its attempt to show the range of problems that a theory of motivation must embrace, the symposium sometimes suffers the loss of the most meaningful kind of interchange among participants, that which naturally occurs when participants are equally interested and sophisticated in the same problem area. The flavor of the interchange is occasionally lost completely when one or another participant is delinquent about preparing his informal comments for the larger audience.

The special weight given to what is new and different in selection of participants may strengthen the widespread impression among some students of psychology that motivation covers everything from soup to nuts and that there are nearly as many concepts as there are researchers. This criterion has, at least, satisfied the initial intention of the symposium, which, as stated by Jones (1953), was to enhance graduate teaching by selecting a central theme, motivation, "around which a vast amount of experimental data from many divergent sources can be assessed and evaluated." The seven volumes do indeed constitute a provocative set of readings about sufficiently diverse problems to warrant their use, selectively, in courses as remote as physiological and social psychology. The symposium represents one of the more creative uses of training grants offered by the U. S. Public Health Service.

Except for approximately a third of the papers which deal in some way with problems presented by the concept of drive, there is, however, little continuity in the series. Most discussions of other

problems, methods, or concepts are one-shot affairs, usually a report of progress in some new area of research.

THE fate of the concept of drive is the single thread which holds these volumes together. This theme accurately reflects a decade of debate about the adequacy of the traditional treatment of motivation. But omission of affirmative presentations of at least two other currently influential points of view (Lewinian Field Theory and Decision Theory), which offer other concepts for systematic consideration of motivation, must be counted the most serious deficiency in coverage.

From J. S. Brown's (1953) forceful keynote presentation of the fundamental properties of drive in S-R theory, and his unfortunate identification of the study of motivation with the study of drive, to the minimization of the importance of this particular theoretical construct in papers by Bolles, Estes, Littman, and even Spence in the 1958 volume, drive, the energizer, is the center of interest. In earlier volumes, Harlow (1953), Olds (1955), and Young (1955) present the main substance of the conviction that had gained credence during the early fifties that drive reduction is an inadequate explanation of 'reward' or 'reinforcement.' The upshot of this line of criticism, the first major theme, is a penetrating analysis of the distinction between "instrumental behavior" (means to ulterior ends) and "intrinsically regulated behavior" (behavior which is apparently 'satisfying' as an end in and of itself) by Koch (1956).

Koch argues that practically all systematic analyses of motivation have heretofore been phrased in the "instrumental grammar": i.e., the organism responds in certain ways *in order to* bring about some satisfying state of affairs or *in order to* avoid some irritation. In urging a fresh analysis which gives equal emphasis to behavior that has an end-in-itself character, Koch reveals the paucity of knowledge concerning what psychologists have previously treated only as potential incentives for instrumental behavior. Only now that the conceptual barrier of the drive-reduction explanation of reward has been shattered can we begin to view eat-

ing, for example, as merely one instance of a much broader class of intrinsically satisfying behaviors (including also exploration, manipulation, etc.) which needs to be studied in the systematic way that Young (1955) studied appetite and Beach (1956) sexual activity.

A SECOND major theme is introduced by Postman (1953) in a comment on Brown's opening paper, when he calls attention to the complete operational confounding of drive (the presumed "energizer") and drive stimulus (the internal cue) known to be produced by conditions of privation. Postman raises the general question: "Is the concept of drive as energizer needed in behavior theory?"

This problem comes to a head in the 1958 volume when the necessity of a separate concept of drive as energizer of responses is clearly minimized. Bolles comes to this conclusion following a long-needed critical survey of evidence previously advanced to support the construct. Estes, in a major theoretical contribution, shows how the facts of behavioral activation as a consequence of privation can be derived within an S-R framework without introducing a special energizer by making full use of the cue function of internal stimuli. And Spence, in his revisions of S-R theory, now directs attention to the motivating function of incentives, the suggested mechanism for which there is the anticipatory goal reaction (r_g) that can be aroused by either external or internal cues. In proposing that the motivating effects of food privation, which are represented in S-R theory as drive (D), and the motivating effects of the food reward, which are represented in the theory as incentive (K), are additive (i.e., $E = H \times [D + K]$) rather than multiplicative as Hull had previously assumed, Spence is in a position to accommodate the proposal already advanced in the literature that *all* of the effects previously attributed to 'drive' may be mediated by the anticipatory goal reaction elicited by the internal cues which accompany privation. In other words, *all* of the animal's motivation may be what Spence has called "incentive motivation." Readers familiar with S-R theory will note that the

anticipatory emotional response (fear) plays an analogous role as motivator in the case of aversive behavior.

As the importance of drive is minimized by S-R theorists, and anticipatory reactions to an incentive like food or to a noxious stimulus like shock take over as the central influences on performance, it should be more obvious than it generally seems to be that S-R theory is beginning to stress the importance of the very same events that Lewin had captured in the concepts of positive and negative *valence* and that current decision models embrace with the concept of *utility*. In offering many interesting but only partly conceptualized empirical explorations at the frontiers of human motivation, but in failing to include systematic treatment of human motivational phenomena through the spectacles of Lewinian Field Theory or Decision Theory, the symposium has unwittingly encouraged the idea that there is a great discrepancy between current treatment of animal and human motivation when, in fact, the *convergence of systematic concepts* is the most striking and encouraging trend on the current scene.

It is a paradox that so many psychologists are enthusiastic about the possibility that the ascending reticular system in the brain (as described by Lindsley, 1957) may be considered the seat of 'drive' and that some encourage the use of physiological reactions to assess the total strength of 'drive' (Malmö, 1958), at the same time that others who have examined the behavioral evidence thoroughly (Bolles and Estes) have arrived at the conclusion that the construct of drive is superfluous in behavior theory. In this respect, the symposium accurately reflects the dismal state of much of the current literature in psychology, so like a cocktail party where everyone is talking and no one is listening. Perhaps a more fruitful linkage to examine, in light of the trend, is that between anticipatory goal and emotional responses and the separate brain centers of reward and punishment advanced by Olds (1955).

Of course, not everyone has found it useful to consider 'motivation' as equivalent to 'drive.' In the most sweep-

ing attempt at a survey of the diverse ideas about motivation (yet one which sadly neglects Lewin), Littman (1958) is led to the popular conclusion that 'motivation' is simply a generic term which embraces a heterogeneous group of phenomena having only "trivial communality." He despairs of finding any fruitful psychological basis for rationalizing past, present, and future motivational concepts. To make his point, he facetiously offers a "comprehensive" definition of motivation in a single sentence that runs, by my count, to 244 words. Perhaps the problem is that some of the blind men who think they have hold of part of the elephant are really shaking a tiger by the tail.

More important contributions of Littman's survey are these: He identifies the "trivial communality" among the 52 terms he has listed as words traditionally considered motivational; viz., they all refer to some "active," energizing principle. Then he identifies the fallacy in the assumption that some variables, like *drive*, are *activators* of behavior while other variables, like *habit*, are *passive* or *inert dispositions* until they are energized.

Considering a skeletonized version of Hull's famous equation for reaction tendency, $r_E = f(sH_r \times D)$, Littman points out that if sH_r has a value of 0, there will be no reaction regardless of the value of D . Given a certain level of drive (D), the resultant behavior depends upon the particular habit (sH_r) and its strength. "And what I mean 'depends' is simply that sH_r activates the total system to yield a response of a particular character; if everything else is equal, then the response is uniquely and totally dependent on sH_r ."

Having cleared away this much debris, it is a shame that he does not then carry the argument to its logical conclusion: to wit, any variable which influences the momentary strength of a tendency to respond in a certain way should be considered a 'motivational' variable. The theoretical problem of motivation is to discover what different variables need to be taken into account and how they combine (or interact) at an instant in time to determine the strength of the tendency which is manifested in the direction (selection), magnitude, and per-

sistence of a particular response. These three dependent variables constitute the behavioral problem of motivation. Marshall Jones comes close to hitting the mark in his first tentative attempt (1955) to describe the broad problem of motivation which seems to be of interest to everyone: "how behavior gets started, is energized, is sustained, is directed, is stopped, and what kind of subjective reaction is present in the organism while all this is going on."

Certainly what Spence (1958) is doing in revising the *contemporaneous* principle in S-R theory is proposing (a) that certain kinds of variables need to be taken into account, and (b) that they interact or combine in a certain way to influence the momentary strength of tendency to respond.

The over-all trend in the evolution of S-R theory is clearly stated in the conclusions of Bolles (1958). They are of sufficient general interest and importance to quote:

- (1) Where little is known about the conditions that control some behavior (e.g., exploratory behavior), the inference of a causal drive does little more than restate the facts to be explained.
- (2) As more becomes known of the details of some behavior and the factors that control it (e.g., sexual behavior), stimulus conditions are found to play a dominant role, and the drive concept diminishes in importance.
- (3) The generalized drive hypothesis, which could provide a bulwark of drive theory, is supported by only fragmentary evidence. Where the evidence does support the hypothesis, it is also interpretable in terms of a stimulus control principle.
- (4) Several theorists have found it convenient, if not necessary, to postulate an energizing function for r_0 , and to give r_0 a place alongside of D as a habit-strength multiplier.
- (5) The most crucial question here for the drive theorist is whether there is any possible way to distinguish between the energizing function of drives and (a) the response-eliciting power of the stimuli that must be assumed to accompany drives and (b) the energizing function of r_0 .

FOLLOWING this climax of interest in drive, the 1959 symposium turns to developmental aspects of motivation. Hess reviews evidence from his studies of 'imprinting' which accentuate the importance of 'primacy,' 'the law of effort,' and 'critical period.' Levin and Baldwin

present and analysis of pride and shame in children that is theoretically neutral with respect to issues concerning achievement motivation and test anxiety to which their work is obviously related. Whiting traces the child-rearing antecedents of three methods of social control in different cultures. And in the only paper not related to some developmental problem, Janis takes issue with some of the ideas in Festinger's dissonance theory that concern resolution of decisional conflicts.

Harbingers of significant changes in orientation may be contained in the 1959 papers of Schneirla and Cattell. As the importance of the concept of drive diminishes in the S-R analysis of appetitional and aversive motivation there may be greater interest in the conclusion Schneirla reaches in a broad comparative and developmental survey: the fundamental categories of all motivated behavior in all animals are 'approach' and 'withdrawal' reactions. Originally, these biphasic reactions are elicited by weak and strong stimulation, respectively. Later, the anticipative 'seeking' and 'avoidance' which develop in higher forms may have little relation to the simple, quantitative characteristics of a stimulus.

Portions of Cattell's long chapter on the "dynamic calculus" come through as the most clear and insightful presentation of his views in the psychological literature. At his worst in an undocumented harangue against American psychologists for burying (I take it he means 'alive!') the concept of instinct, Cattell more than compensates in some sharply focused proposals for the use of multivariate analysis in conjunction with experimental procedures in animal research. He sees that this, alone, will provide both an unambiguous demonstration of the power of his method and also the much-needed opportunity for growth of insight concerning what sorts of inferences can be legitimately made from factor analysis. This type of research is required to support the new interpretations he offers of the components of motive strength and his conjectures as to how they combine to produce resultant "ergic tension." If some experimentalist will only meet Professor Cattell half way in this methodological

innovation, the consequence could be a final cracking of the barrier between 'correlational' and 'experimental' psychologies which has isolated the measurement of individual differences and the principles concerning the fundamental process of motivation.

Were the *Nebraska Symposium* to do no more than contribute in this small way to the elimination of an unfortunate historical dichotomization of interest and of competence among psychologists, it could be counted a significant contribution.

Tense and Happy Workers

Floyd C. Mann and L. Richard Hoffman

Automation and the Worker: A Study of Social Change in Power Plants. New York: Henry Holt, 1960. Pp. xvi + 272. \$4.50.

Reviewed by HAROLD J. LEAVITT

Dr. Mann, the senior author, has been with the University of Michigan's Survey Research Center since 1947 and is now a program director there. Dr. Hoffman, until recently a research psychologist in the Willow Run Laboratories, is now a research associate and lecturer in Michigan's Department of Psychology. The reviewer, Dr. Leavitt, is Professor of Industrial Administration and Psychology in the Graduate School of Industrial Administration in the Carnegie Institute of Technology in Pittsburgh. He is the author of Managerial Psychology (Univ. Chicago Press, 1958).

MOSTLY this book is a research report about the effects of automation on jobs and attitudes, though periodically the research results are interspersed with pleas for more social science in industry and attacks on old Mr. Taylor.

The research is about the attitude of employees and supervisors in two power plants. The measurements are taken with two long questionnaires administered to

supervisors and operators. The two plants differ. One is newer and more automatic. It is also rural, physically attractive, and staffed by a smaller work force, carefully selected from the other plant, with management more conscious of human relations, also carefully selected from the older plant. The other is an old and less automatic unit. It is urban, more run-down, with a bigger and older work force, a more autocratic management, and a history of labor strife. The authors, therefore, encounter considerable difficulty in trying to isolate the effects of automation from the effects of these other variables.

Besides this confounding that plagues so much field research, the book itself suffers from old age. The data were gathered late in 1954, but the publication date is 1960. This is probably the biggest reason for my feeling that nothing very new or stimulating could be found here. Neither the research results, nor the pleadings, nor the methods seem novel in 1960.

On the other hand, the fact that the authors had the wisdom to spot this critical field early stands in their favor, as does the fact that they have collected empirical data, albeit a little old and confounded. Though much has been written about automation and though labor contracts and company policies and arguments have been forged around the issue, facts have been hard to find.

If we therefore pass on beyond the problems of old data and the confounding of results, we arrive at the substantive findings of the study, which may be of considerable interest to many readers.

Essentially the authors find that employees at the new plant like their jobs better than employees at the old plant. They attribute the new plant's higher morale to the relative enlargement of the men's jobs, an enlargement dictated, they feel, by the automatic technology of the new plant. They found, too, a greater readiness to accept changes (of job design, etc.) in the new than in the old plant, though it is not clear that advanced technology was a significant variable here. At least it seemed to me that the absence of a history in the new plant as well as the self-selection of employees were more likely causal factors.

Supervisors at the new plant behaved better in respect of human relations than at the old. They gave more local autonomy to lower levels of personnel—though automation again seems to have been only one of several factors that may have contributed to this difference.

An unusual finding: Workers in the new plant felt somewhat more tension than in the old, despite their higher morale. The tension is, however, attributed to too speedy and incomplete training for the new and bigger jobs, rather than to increased size and complexity of jobs; the authors, therefore, do not regard it as an effect of automation.

Some interesting findings about shift work turn up, and some of them are related to automation. Shifts in the new plant leave men more alone than in the old. Just a few men operate all the equipment on shifts, so that initially, at least, shift supervisors feel somewhat troubled about the scope of their lonely responsibility.

The book has an uneven quality, due, I think, to periodic shifts from reporting and analyzing to pleading and attacking. Pleas at the beginnings of chapters always seem especially inappropriate in a research report. Since they precede their exposition, they cannot be excused as interpretations of the data. Such a plea appears at the beginning of Chapter 4. It takes the form of an attack on Taylor's Scientific Management (circa 1911) and on industrial engineering in general. Some of its phrasing added to my feeling that I was reading a book from the past. The argument was reminiscent of the attacks of the earlier 1920s on Taylor and the robber barons; e.g., "Increased productivity [as a consequence of scientific management] has been obtained at the expense of condemning workers to spend their working lives at repetitive, monotonous, and intellectually deadening jobs" (p. 68).

If we can forgive the loaded wordings, and some of the other difficulties of the book, it is worth noting that, in this case at least, a new automated plant is associated with job enlargement, more mutual consultation and communication, and higher morale, rather than with further job reduction and routinization.

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Contemporary Experimental Psychology of Perception

William N. Dember

The Psychology of Perception. New York: Henry Holt, 1960. Pp. xiv + 402. \$6.50.

Reviewed by RICHARD H. HENNEMAN

The author, Dr. Dember, having taught psychology at the University of Michigan and at Yale, has recently received his PhD from Michigan and become Assistant Professor of Psychology at the University of Cincinnati. He is a young man who has now written an important and timely book. The reviewer, Dr. Henneman, is Professor of Psychology at the University of Virginia where he has taught since 1947. In 1935 he was a Woodworth PhD at Columbia with a thesis on perceptual constancy. At various times he has worked on perception with F. A. Geldard, J. P. Guilford, and J. J. Gibson, and his current research has to do with the biasing of response in ambiguous perceptual situations.

DEMBER'S *Psychology of Perception* is a refreshing and stimulating new book on one of the oldest areas of psychology. The approach is eclectic, experimental, and contemporary (concerned largely with concepts and problems in the research literature since 1950). There is little said about the historical and traditional problems of perception. The author does not emphasize a particular determiner of perception, as Gibson did in his *Visual World*, nor does he follow Allport in surveying the various theories. And he has not, like Bartley, attempted a comprehensive coverage of the entire area. Rather, he has sampled from the extensive field of perceptual phenomena.

In the preface the author admits to several biases influencing his selection of topics and the relative emphases given to each: a concern for the problems of measurement, and special interests in visual psychophysics, in the effects of

learning on perception, in observer variables influencing perception, and in curiosity and exploratory behavior. The topics are chosen almost wholly from the visual sense. This emphasis is defended on the grounds that man is primarily a 'visual animal' and vision affords a greater range and variety of phenomena to study. In this reviewer's opinion, Dember's selected topics meet the following criteria: (1) those which fit into an integrated, coherent area of investigation; (2) those best illustrating the various conditions which significantly influence perception; (3) those revealing the relation of perception to other areas of psychology (e.g., learning, motivation, emotion); (4) those which offer the best promise of fruitful and valid future research.

In his first chapter Professor Dember sketches a very general definition of perception to provide a framework for the topics to be presented subsequently. Regarding the behavior of the organism as *output*, and stimuli impinging upon the organism as *input*, *perception* is considered as relating output to input. Output and input are potentially observable; the intervening perceptual process is not. "The task of the psychologist who is seeking the principles of perception is the task of making inferences, or guesses, about relations, given knowledge of the variables that enter into the relation." Dember does not specify the nature of the perceptual process as such, being content to treat perception as a "set of intervening variables" related to observable stimulus conditions and to observable behavior patterns. This point of view is in line with contemporary thinking, as

exemplified in the notions of Garner, Hake, and Eriksen (1956).

MORE arresting and suggestive than the author's formal definition of perception is his analysis of the basic perceptual processes—*detection, discrimination, recognition, and identification*. Very carefully he defines each of these processes, providing specific examples; then he proceeds logically and empirically to derive recognition and identification from the more elementary processes of detection and discrimination. This presentation is similar to that which Arnoult has advocated and to the treatment in Gagné and Fleishman's recent introductory textbook. Such clarification in conceptualizing the fundamental processes has been greatly needed in perceptual research and Dember has added a very valuable emphasis here. *Stimulus change* is stressed as an essential condition for any perceptual process. This point is emphasized again in the final chapter when stimulus change is proposed as a key factor in motivation and emotion.

In discussing the traditional division between sensation and perception, Dember agrees with Gibson, Allport, and Bartley in rejecting the validity of the distinction—and a very successful integration he achieves. Starting with the development of perceptual processes from simple detection thresholds to the identification of meaningful stimulus complexes, he emphasizes throughout the book the point that the same basic processes are involved in perceptual judgments of all kinds. The reader gains the impression that this area of psychology could not adequately be treated in any other fashion and well might wonder why an effective coordination of these topics has proven to be so difficult in the past.

There are three early chapters on experimental methodology and measuring techniques. Two aspects of the procedure for measuring thresholds are stressed: the nature of the *indicator response* employed, and the *method of selecting the values of the stimuli*. The important point is that the obtained results are in part a product of the methodology. Methods of stimulus scaling are discussed in a very interesting chapter which describes some of the recently

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developed techniques and their research applications.

Contextual variables are assigned the importance which they deserve, but have seldom received, as determiners of perception. Context is viewed broadly to include both spatial-simultaneous effects and temporal-successive influences such as figural aftereffects and adaptation level. Furthermore, context is presented as including not only interrelations within a single sense modality (e.g., contrast, illusions), but also intersensory phenomena, extending to the Werner and Wapner researches on their *sensory-tonic* theory of perception. Context is thus not only a highly significant determiner of perception, but also a very far-reaching one. Nowhere in his book has Dember more strikingly portrayed the essential interrelationship and unity of phenomena which in the past have too often been treated separately as 'sensory' or 'perceptual.'

THE troublesome problem of the effects of learning on perception is treated in advance of a consideration of the influences of set and motivation. The larger part of the learning chapter is devoted to studies of the effects of early experience, and of the deprivation of normal experience, on the development of perceptual processes. There follows a consideration of discrimination training and the influence of learning on complex perception, as exemplified in the perceptual demonstrations of the transactionalists. Dember concludes: "That learning enters into the determination of perception seems unquestionable; exactly where in the system this happens, however, has not yet been established." In discussing the role of set in perception he proposes that "set may be thought of as a type of context provided by cognitive conditions rather than simple stimulus conditions. Or from another point of view, set can be considered as analogous to the instructions that determine the manner in which a computer handles its input." Dember gives detailed and critical consideration to the studies of differential word frequency as representative of the operation of set on perception. He makes the important point that the results of these studies may be the products of biases

built into the *response indicator*, or "stimulus utterance," rather than a genuine manifestation of altered *perception*.

Consideration of the effects of motivational variables on threshold and sub-threshold phenomena leads the author to a critical examination of 'perceptual vigilance' and 'perceptual defense,' and so to the intriguing question of 'perception without awareness.' He notes again the problem of response biasing as it plagues research in this area of perception. In attempting to account for the paradoxical findings that the individual



WILLIAM N. DEMBER
and daughter Joanna

can respond emotionally to stimuli which cannot be perceived, he suggests that we may "have learned to react emotionally to subtle, fragmentary cues, but to ignore these same cues in making rational, precise judgments about external stimuli." Thus "perceptual defense and subliminal perception represent, not paradoxically low thresholds as measured by emotional responses, but rather spuriously high identification thresholds as measured by the conventional psychophysical procedures."

THE concluding chapter is frankly speculative, but the suggestions are derived from available experimental evidence, some of it from the author's own research on exploratory behavior. Dember's thesis is that perception significantly contributes to both motivation

and emotion. First, one sees that stimulus change is a strongly motivating condition for both animal and human behavior. Stimulus complexity, defined in terms of how much the individual can do with the information presented, acts in essentially the same manner as stimulus change. There is an ideal or preferred level of stimulus complexity, and the individual is most responsive to stimuli at this level. Stimuli below this level lead the individual to seek stimuli of greater complexity. Stimuli of excessive complexity are avoided. In relating perception to emotion, the studies of sensory deprivation are cited as evidence that forced restriction of stimulus input leads to emotional disruption. On the other hand, stimuli thrust upon the individual that are above his "ideal level of complexity" may elicit from him emotional reaction ranging from mild surprise to terror or rage. Although these notions are but tentatively expressed, fascinating vistas of future research emerge in this final chapter.

Dember states in his preface that his book is intended as a text for a one-semester laboratory course in perception. In the opinion of this reviewer he has succeeded admirably. He writes clearly and forcefully. His presentation of concepts is clarified by specific examples, both anecdotal and experimental. The multitude of studies referred to provide many excellent experiments for demonstration in the lecture room and performance by students in the laboratory. Dember's emphases on the contemporary and experimental aspects of perception make the subject seem 'alive' and a fascinating area for study, and he evidently believes that the day is not far off when adequate experimental methods will be available for conducting sound research on many of the complex and significant perceptual problems which have so long defied valid scientific investigation. If his book hastens the arrival of that day, he will indeed have made a very significant contribution to psychology.



We may have ideas of which we are not conscious.

—JOHN NORRIS (1632-1704)

An Elementary Text in Child Psychiatry

Stuart M. Finch

Fundamentals of Child Psychiatry. New York: W. W. Norton, 1960. Pp. 334. \$5.95.

Reviewed by JOHN J. CONGER

The author, Dr. Finch, is identified in the review. The reviewer, Dr. Conger, is Head of the Division of Clinical Psychology in the School of Medicine of the University of Colorado, where he trains clinical psychologists and does research in child development. For a little more about him, see his review of Beck and Molish's Reader in Clinical Psychology (CP, Sept. 1960, 5, 309f.).

CHILD psychiatry has long needed a good introductory text. There have, of course, been several comprehensive summaries of the field—most notably Kanner's pioneering *Child Psychiatry*—as well as numerous less exhaustive or more specialized contributions, like those of Lippman's, Erikson's, Pearson's, and Anna Freud's. But while each of these has proved helpful for particular purposes, none has adequately met the beginner's need for a clearly written, well-balanced text, providing both a broad overview of the contents of the field and a reasonably systematic theoretical frame of reference within which more specialized knowledge can be profitably pursued.

Dr. Finch has attempted to fill this void. He is Associate Professor of Child Psychiatry and Director of Children's Psychiatric Hospital at the University of Michigan. He is co-author, with O. Spurgeon English, of *Introduction to Psychiatry*. He has long worked with clinical psychologists and shows a refreshing understanding of the contributions that well-trained ones can make to clinical practice.

The author of any general text in child psychiatry faces a much more diffi-

cult task than his counterpart in adult psychiatry. One reason, of course, is that development during childhood is so fluid and rapid. Dominant influences at one age may become of minor importance at others. The degree of ego integration expected at two is vastly different from that at seven. Symptoms which are common at some ages may be pathognomonic at earlier or later ages. Simply solving the problem of orderly exposition in the face of such chronological diversity presents a considerable challenge.

Another potential stumbling block is found in the problem of nomenclature and classification. While adult psychiatry has come to terms—however tentatively and uneasily—with this problem, child psychiatry has not.

Consequently, the author of a text in this field has to thread his way between the advocates of taxonomic nihilism and the diagnostic name-droppers who are inclined to elevate every symptom, from enuresis to refusal to eat spinach, to the status of a separate etiological entity.

Further, in the case of introductory texts like Dr. Finch's, the author has to reckon with varying degrees of the readers' sophistication in general personality theory.

In the face of such formidable challenges, it is not surprising that Dr. Finch has met them with varying degrees of success.

One of his welcome assets is his ability to write clearly and simply, with a minimum of technical jargon. In addition, he has worked hard to present his material in an organized, orderly fashion—beginning with a brief chapter on per-

sonality development, and proceeding to considerations of etiology, classification, parental psychopathology, history taking, psychological examination, treatment techniques, and special problems, such as adoption and divorce.

Useful case illustrations are sprinkled throughout the book. Typical symptomatology is simply and clearly described, and current notions of etiology are adequately—if briefly and at times a bit dogmatically—presented.

THE outcome of the author's attempts to wrestle with the problem of classification will doubtless lead to spirited controversy among professional readers, despite its middle-of-the-road approach. Dr. Finch settles for making initial divisions by age, while noting "the fact that youngsters are constantly changing and do not show the static personality syndromes seen in adults is one reason why the problem of nomenclature . . . in children has never been satisfactorily resolved."

Within each age period, Dr. Finch's chief distinctions lie between psychoneurotic (or pre-neurotic in earlier years), personality, psychophysiologic, and psychotic disorders, with traditional subdivisions where they appear appropriate. Generally, he has tried to follow the format of the APA diagnostic manual—a process which, while helpful in promoting organization of the material and avoiding confusion, becomes at times somewhat artificial and forced, as the author himself recognizes with respect to differentiating "transient" and "permanent" problems in growing children.

While appreciating the problems involved, the reviewer was troubled by Dr. Finch's necessarily condensed exposition of the psychoanalytic theory of behavior. In the initial chapter on personality development, psychosexual development, personality structure, and the mechanisms of defense are summarized in fourteen pages. Such brief distillations add little to the advanced reader's store of knowledge, while running the danger of alienating the novice with what may appear as a narrow and dogmatically arbitrary view of the complexities of human development.

Further, there is relatively little em-

phasis in this rather traditional book on more recent contributions to our understanding of personality from such areas as psychoanalytic ego-psychology, child development, or the other behavioral sciences. The discussion of adolescence appears particularly thin in this regard.

Taken as whole, however, this is a creditable and a useful, if not an exciting, introductory text in a difficult and sparsely populated area. While it offers little that is new or challenging, it is simply and effectively written, clearly organized, and presents a reasonable overview of the field. Further, the organization of the book is such that it can be easily supplemented by additional reading or lectures.

School Psychology Goes Professional

Monroe G. Gottsegen and Gloria B. Gottsegen (Eds.)

Professional School Psychology.
New York: Grune & Stratton,
1960. Pp. x + 292. \$7.75.

Reviewed by MARVIN POWELL

The editors of this volume are identified in the review. The reviewer, Dr. Powell, is Professor of Educational Psychology at Northern Illinois University and Coordinator of Psychological Services in the Willoughby-Eastlake Board of Education. He reviewed Loe's Educational Psychology for CP (Oct. 1959, 4, 336).

DESPITE the increasingly rapid growth of the field of school psychology, it has been surprising to find so little written about the field other than the Thayer Conference report of a few years ago. For this reason the reviewer, involved in so many different phases of the field, was particularly interested when he heard of the publication of this book.

The editors seem to have the types of training and experiential backgrounds to enable them to do a thoroughly

competent job of assembling the articles of which this book consists. Dr. Monroe Gottsegen was a school psychologist at the Bureau of Child Guidance and Bureau of Educational Research of the New York City Board of Education and is now a clinical psychologist with the Veterans Administration at the Brooklyn VA hospital. He is also a consultant in child therapy at the Jamaica Center for Psychotherapy. His wife, Gloria B. Gottsegen, was formerly a clinical psychologist at the Developmental School for Disturbed Children in New York City and is now Consulting Remedial Psychologist for the Jewish Child Care Association in New York.

They have chosen for inclusion in this book articles by well-accepted authorities in the various areas discussed, although none of the authors seems to be functioning as an active school psychologist. The authors are, for the most part, college professors, educators, psychologists, and psychiatrists who work in clinic settings rather than in an actual school environment. One may, of course, safely assume that they deal with many school-related problems and probably often have direct contact with teachers and other school personnel; yet this reviewer would have preferred to have had some of the articles written by school psychologists working in the field.

In the preface Dr. Gottsegen states: "This volume is addressed primarily to school psychologists in current practice, but it is addressed as well to those graduate students who are interested in acquiring some familiarity with the concerns and problems of this profession relative to their own futures within it." The reviewer questions the value of this volume to school psychologists in cur-

rent practice. If their training was adequate (often it is not), they must be sophisticated beyond the level of information being offered. Of course, other psychologists may get from the book an idea about some areas of a field strange to them, but at best they would get but cursory glimpses. The book might, indeed, be of some value to a graduate student contemplating a career in this field. It might even make a good textbook in an introductory course that tries to give an overview of many fields, but certainly it would not be the much-needed text for students well along in their special training.

In general, it may be said that this book offers a fairly comprehensive coverage of the various areas within school psychology, but that the articles are too brief to offer sufficient depth. The reviewer was pleased to find articles dealing with *Psychotherapy with the Adolescent* and with *School Phobia*, since these are two areas that have often been neglected in the training of school psychologists.

It seems to this reviewer that the Gottsegens have achieved two goals. They have in the title and content of this book offered long-due recognition to school psychology as a profession. They have also produced a book which might well be read by psychiatrists and general psychologists to gain a better understanding of the great difficulties and the tremendously varied number of tasks with which the school psychologist must work. These are noteworthy achievements, yet they leave most of us involved in the field still waiting for that text with comprehensive technical coverage.



At school you are not engaged so much in acquiring knowledge as in making mental efforts under criticism. A certain amount of knowledge you can indeed, with average facilities, acquire, so as to retain, nor need you regret the hours spent on much that is forgotten, for the shadow of lost knowledge at least protects you from many illusions. But you go to a great school not so much for knowledge as for arts and habits; for the habit of attention, for the art of expression, for the art of assuming at a moment's notice a new intellectual position, for the art of entering quickly into another person's thoughts, for the habit of submitting to censure and refutation, for the art of indicating assent or dissent in graduated terms, for the habit of regarding minute points of accuracy, for the art of working out what is possible in a given time, for taste, for discrimination, for mental courage and mental soberness.

—WILLIAM (JOHNSON) CORY

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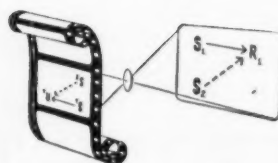
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INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



PROGRAMMED TEXTBOOKS IN STATISTICS

IN this issue INSTRUCTIONAL MEDIA presents two reviews of auto-instructional programs applicable to introductory courses in statistics. Both programs are the work of the same authors, and both are in programmed textbook form, in which confirmation or correction of the student's response is provided by his turning to the next page in the book immediately after responding to each frame of the program—rather than through the use of a 'hardware' teaching machine to provide more complete stimulus control over the sequencing of the material. The earlier program, *Descriptive Statistics*, appears to be the first published program in the constructed-response, Skinnerian style, and, as such, it has special interest in the rapidly growing field of auto-instructional programming. It closely antedates Harcourt Brace's publication of a program in English usage and composition (to be reviewed in next month's issue), and a preliminary form of it has been previously reviewed by Dewey Slough in *The Psychological Record* (July, 1960). Its formal publication and general availability for sale also preceded that of the already well-known program on the science of behavior by J. G. Holland and B. F. Skinner—a program which, after having been widely used in preliminary form, is soon to be published by McGraw-Hill and will be reviewed in an early issue of *CP*. It is to be hoped that Galanter's critical review will stimulate interest and controversy concerning the authors' approach to the subject matter of statistics, over which there appears to be considerable disagreement among teachers of statistics, and that the concern of both reviewers with program try-out and standards of assessment (a concern certainly shared by the authors

of the programs) may help to illuminate both the importance and the difficulties attending this crucial aspect of program development. The editor of this department would like to note that, despite the quite pertinent criticism of the programs made by Galanter and Saltzman, both have been found to be very useful by a number of individuals in providing unsupervised self-instruction in statistical methods.—A. A. L.

Common Statistics Uncommonly Taught

James L. Evans, Lloyd E. Homme,
Robert Glaser, and Charles J. Stetler

*Descriptive Statistics: Volume 1 of
a Program in Statistics.* Albuquerque, N. M.: Teaching Machines, Inc., 1960. Pp. 430. \$10.00.

Reviewed by EUGENE GALANTER

Dr. Galanter professes psychology at the University of Pennsylvania, where he got his PhD in 1953. While at the Center for Advanced Study in the Behavioral Sciences he edited a book of papers on self-instructional programs and techniques, *Automatic Teaching: The State of the Art* (Wiley; CP, Apr. 1960, 5, 104f.). In collaboration with G. A. Miller and K. H. Pribram he has also recently produced *Plans and the Structure of Behavior*, which was published by Holt in 1960. For the past couple of years he has been active in the development and testing of programmed material employing questions in multiple-choice format for various school subjects.

My friends who make it their business to select executives say it is a two-dimensional problem: brains and aggressiveness. The four cells of the 2×2 predictor table are smart-aggressive, stupid-aggressive, smart-passive, and stupid-passive. Avoid at all costs the stupid-aggressive cell, they say. Instructional media have a similar structure with "effectiveness-in-teaching" and "quality-of-content" as dimensions. The dangerous cell is the "effective" cell with "poor content." The teaching-machine revolution has generated auto-instructional media (programs) of testable effectiveness. Intuitions about how effective a text will be are no longer the province of the reviewer. In principle, the author(s) can supply factual information. Just as a new psychological test is packaged with reliability and validity information, so also can auto-instructional media be supplied with data about effectiveness. To date, however, no standards have been established, so we can only regret that the authors of *Descriptive Statistics* do not include these data and thereby set some standards.

The program in *Descriptive Statistics* is the work of James L. Evans and Lloyd E. Homme, both of Teaching Machines, Inc., and Robert Glaser and Charles J. Stetler, both of the University of Pittsburgh. Writing programs is not easy, and in competition with the climate in Albuquerque, N. M., the home of Teaching Machines, Inc., it is harder still. But we do have this program, and it is a monumental job.

THE volume under review is a "programmed textbook," that is to say, it is both a teaching-machine program and a teaching machine. In use, the student reads an item on page n , writes his answer in a space provided in the item, and then goes to page $n + 1$ to check the "correct" answer. Page $n + 2$ contains the next item. Each page contains a number of items, but all the items at the top of each page are worked through before going on to the items in the middle of each page, and so on.

The form of the program is in the Skinner-Holland tradition, that is to say, items are introduced in an order that aims at a minimum of errors. The

development of concepts is slowly approximated, and the student is led to abstractions through concrete steps. The items appear to be carefully selected, but seemed to me to lack the requisite redundancy and transformed iteration that substitutes for the constant repetition of classical drill. The authors show skill in their selection of the responses that are demanded of the student. Only rarely do they lapse into items that require responses unessential to the repertoire they are trying to shape.

There are about 825 items to complete, which means that construction must have taken something of the order of 1,000 to 2,000 hours. A person working full time writing items would need about a year to do the job. But that is only the beginning. As Thomas Gilbert has pointed out, auto-instructional media must be edited by the student. We have no theory for the construction of items at present, and so constant monitoring by the ultimate consumer is necessary. The item sequencing appears to be reasonable, but one never knows except by a trial. The results of such a trial are hard to interpret, but, if the number of student-errors is large, we tend to question the adequacy of the program to teach.

A trial of the program would take a student from 10 to 30 hours; thus it is hard to run a test. Happily, a volunteer appeared to serve as a student. The results for the entire experimental population are:

Time per item:	ca. 55 sec.
Probability of correct response	ca. 0.93
Post-test grade:	B+

To interpret these results, we first must understand that the program is divided into ten "books" or units, one each on: basic concepts, frequency distributions, measures of location, and measures of dispersion; and two each on the normal distribution, variance calculations, and correlation. The early parts of the program teach definitions and the late parts give problems to solve. Therefore, the error rate, through calculational errors, grows as the student progresses. These errors would, presumably, be eliminated by adequate elementary preparation, but unhappily,

our subject suffered under a pre-teaching-machine educational system.

The reported post-test grade was calculated by subjective integration of the student's ability to solve problems and to understand statistical concepts, minus her native talent and sophistication. The results of this experiment lead me to categorize the auto-instructional media and the device for presenting it as "effective-for-teaching." And so we turn to the question of content. Will it teach "good" statistics, or "bad."

THE use of graphs to develop geometrical intuitions about mathematics is a good idea, and one of decided practical advantage in statistics. But if the graphs are used without regard for precision, the standards of rigor can become less than desirable. This fault recurs throughout *Description Statistics*. Some examples will make the point.

Histograms are shown that indicate frequency at a point, but the width of the bar, extending as it does over an interval, leads the serious student to confusion. Class intervals are discussed later, but are irrelevant to the idea that at a point the bar width is arbitrary. Graphs that purport to show that two distributions have the same mean but different ranges tail off indeterminately. In fact, anyone at all sophisticated is forced to conclude that the graphs have the same *range* but different *variance*. In addition, these little figures have no scales on them, and, if the idea of scale transformations is not brought in, erroneous conceptions will be developed. This feature adds even greater confusion when graphs are used to demonstrate that a symmetrical bell-shaped function is not normal because its inflection points are *too close* to the mode. The visual effect may be there, but the area between the inflection points is close enough to 68% to lead to confusion rather than illumination.

Another point at which the student suffers is that the graphs in the text are sometimes referred to as "curves," and sometimes as "distributions." The tendency of the student is to equate these terms. He will then be shown a curve that could not be a distribution function and consequently another wrong idea is effectively taught. In fact, the

normal distribution is called, on first introduction, the "normal curve."

The over-all impression of the content is one of fuzziness or looseness. Few items can be singled out as wrong, but the whole picture is one of ambiguity. For example, the variance is defined as $s^2 = \sum(X - \bar{X})^2/N - 1$. This is the correct definition for the unbiased estimate of the population parameter. A great deal of time is spent discussing the numerator, and we must suppose that the student sees quite clearly into the structure of this component of the equation. The denominator is never mentioned, and, with the student, we are left to wonder about the "-1." In one item, in fact, the formula for the maximum likelihood estimate of the population parameter $s^2 = \sum(X - \bar{X})^2/N$, is scored as incorrect.

IN the opinion of many statisticians and psychologists, the single most important idea of descriptive statistics is estimation. It rests on the distinction between a sample and a population, and the correlative distinction between a statistic and a parameter. Nine items of the 825 in this program are devoted to part of this distinction. The distinction is made only because the concept of inference is introduced. The notion is that the student draws a sample, makes certain calculations that yield statistics, and then makes inferences about the population. Exactly what he infers about the population is never quite clear, nor is his mode of inference characterized. How much simpler it would have been to introduce the distinction between a parameter and a statistic and the notion of estimation. That this can be done at the elementary level is demonstrated by the existence of the second edition of *Introductory Probability and Statistical Inference for Secondary Schools: An Experimental Course* (E. C. Douglas, F. Mosteller, R. S. Pieters, D. E. Richmond, R. E. Rourke, G. B. Thomas, Jr., and S. S. Wilks. *Introductory probability and statistical inference for secondary schools: an experimental course*. New York: Commission on Mathematics of the College Entrance Examination Board, 2nd ed., 1958). In this book the authors devote a chapter to the idea of estimation and the distinction between

a statistic and a parameter. Unofficial reports that I have heard indicate that this concept is not beyond the grasp of the average high-school student.

The major difficulty with the program seems to be that it has been constructed by psychologists rather than by statisticians under the guidance of psychologists. We have not yet established the appropriate partitioning of the job of preparing an auto-instructional medium in a particular subject-matter area. It seems reasonably evident that, at the beginning, psychologists are going to have to do the programming because they know about the techniques; but in short order, we must address ourselves to a search for subject-matter experts who can be taught these techniques and who will devote the effort necessary for the production of a program. In sum, there is danger that this program may be too effective for its content.

Road Under Construction

Teaching Machines, Inc.

Statistical Inference: Volume 2 of a Program in Statistics. Albuquerque, N. M.: Teaching Machines, Inc., 1960. Pp. 187. \$10.00.

Reviewed by IRVING J. SALTZMAN

who got his MA at Duke and his PhD at Johns Hopkins in 1948, just before moving to Indiana University, where he is now Associate Professor of Psychology. His work with animal learning with Koch at Duke evolved into an interest in incidental learning in human subjects and in turn, through a year which he and L. E. Homme spent working with Skinner and others at Harvard in 1956-57, into an active concern with teaching machines. He is currently working on an auto-instructional program in Russian and has recently participated in two national conferences concerned with teaching-machine applications.

THE fact that this review appears in the INSTRUCTIONAL MEDIA of CP and not up front, should suggest, cor-

rectly, to the reader that the primary concern of the reviewer in preparing this report was not with the book as a textbook in elementary statistics, but rather as a new teaching device, as a *programmed* textbook. *Statistical Inference* is Volume 2 of a Program in Statistics; Volume 1 is titled *Descriptive Statistics*. It is assumed that the student who plans to use *Statistical Inference* already has a knowledge of the concepts and techniques which are presented in *Descriptive Statistics*. Neither volume, by itself, is the equivalent of a textbook in elementary statistics. The two volumes together, however, deal with most if not all of the topics usually covered in a textbook in elementary statistics.

A commonly expressed fear with regard to the use of auto-instructional devices is the one concerning the possible detrimental effects on the learner of the cold, impersonal atmosphere of the learning situation when such devices are used with little or no active participation by a live teacher. The reduced opportunity for personal interaction between the teacher and the student, with the consequent loss of the alleged beneficial effects of said interaction, has often been decried. It has required substantial polemic skill on the part of the proponents of automated teaching devices to reassure such critics and persuade them to adopt an unbiased, unemotional, empirical, wait-and-see attitude. In accomplishing this, the advocates of the use of the new techniques, to be sure, have had inestimable assistance from the threats of great shortages of qualified teachers and from the pressures of international competition. It is rather unfortunate, therefore, to find that one of the first programs to appear in the field provides new substance for the fears. The criticism of the impersonal nature of the learning situation can now be directed justifiably to the programming itself. *Statistical Inference* is authored by a corporation. No person is listed as the author. The assistance of Dr. Robert M. Morgan of the University of New Mexico in the preparation of the program is acknowledged, but the specific nature of Dr. Morgan's contribution is not made clear. Four authors are listed for *Descriptive Statistics*.

The author of *Statistical Inference* is: Teaching Machines, Inc. This reviewer hopes that the failure to indicate the names of the authors of *Statistical Inference* is merely an oversight and does not indicate the abandonment of the time-honored custom of identifying authors of books. Certainly the use of the impersonal corporate name in this academic venture is not to be explained as an attempt to obtain the benefits which normally derive from incorporation in the business world, viz., reduced personal liability.

To those who are familiar with *Descriptive Statistics*, *Statistical Inference* will look like a much thinner volume, as indeed it is. However, close inspection of this second volume in the series will reveal that, although there has been a drastic reduction in the total number of pages from 431 to 162, the number of steps or frames is actually a little larger. Whereas the ten units in the first volume contain a total of 823 stimulus frames, the ten units in the second volume contain 828. This reduction in the number of pages without a corresponding reduction in the length of the programmed sequences is accomplished by a fuller use of the pages. Instead of two stimulus frames and two response frames per page, as in *Descriptive Statistics*, *Statistical Inference* has six stimulus frames and six response frames per page. Before the economically oriented reader has a chance to rejoice, it should be pointed out that the benefit to the purchaser of the trimmer model of the programmed textbook lies primarily in the ease with which it can be carried. Volume 2 is lighter than Volume 1, but it, too, costs \$10.00. Although the cost per page has gone up, the cost per frame appears to have remained the same. Unfortunately, however, this price stability is only apparent. Since each page in *Statistical Inference* contains three times as many frames as *Descriptive Statistics*, each page must be turned three times more often per usage than those in *Descriptive Statistics*. Therefore, the expected life-span of the second volume is only one-third that of the first.

Another change in the format of the second volume is the use of alternating black and white, half-inch borders on the outside edges of the frames. This

change was made in an attempt to help the student stay at the same level, in only the literal sense of the word, as he proceeds from page to page through the program. With twelve frames per page and with the size (height) of a stimulus frame approximately twice that of a response frame, it is all too easy for the learner to lose his place each time he turns a page. To reduce the likelihood of such an event, all the stimulus and response frames at a particular level on all the pages of a unit are identified by either the presence or the absence of the black border. On alternate runs through the pages of a unit the student changes from black-bordered items to white ones.

The only other change in the format of the second volume is that the 'panels' are all collected at the back of the volume, rather than being located within the units where they are first used. There are five panels in this second volume: a table of random numbers, a graph of the unit normal curve, a table of the distribution of z , a table of the values of t and a table of the values of chi square. The reader may find it surprising to learn that although Unit 9 is devoted to the *Analysis of Variance*, the volume does not contain a table of the values of F .

STATISTICAL INFERENCE is divided into ten units or topics. Each of the ten units is introduced with a paragraph which describes the subtopics which are treated in the unit. The number of stimulus frames per unit varies from a low of 57 to a high of 132 with a median of 71. Only two of the units have more than 100 frames. The ten topics and the number of frames devoted to each are as follows: Probability—71, Population and Samples—57, Sampling Distributions—78, Hypothesis Testing—65, Testing Hypothesis about a Population Mean—66, The Distribution of z and t —132, Testing Hypothesis about the Differences between Two Means—123, Testing Hypothesis about Variances—71, Analysis of Variance—97, and Uses of the Chi-Square Distribution—68.

It occurred to the writer when he agreed to undertake a critical appraisal of *Statistical Inference* that he might be accepting a difficult assignment. How do

you go about reviewing a programmed textbook? What are the criteria for evaluating this new device? In addition, the authors of *Statistical Inference*, whoever they are, made it doubly difficult. They carefully refrained from making any promises about what the book will do for the student or the instructor who uses it. So there are no wild claims which this reviewer can dispute. Nor are there even any moderate claims. *Statistical Inference* is openly offered as an experiment in progress. In the introduction to the program it is said:

Programmed learning is an ongoing experiment in education. The TMI staff invites both instructors and students to participate in, and contribute to, this experiment. Any techniques, suggestions, or findings or methods of using the Programmed Textbook more effectively, both in its initial presentation and on the review of the concepts presented, will be gratefully received and acknowledged.

The volume even comes equipped with a form which can be filled out with comments and suggestions about specific frames and torn out and mailed back to the TMI staff. Although they report that their program has been carefully prepared, tested, and revised, they admit that it is not in its final form yet. They foresee gradual changes and revisions based on additional student criticisms. From the modesty of their comments about the present form of the program, one might almost expect from them an affirmative answer to the direct question: Did you publish this program prematurely?

Usually the reviewer of a textbook can search for topics which the authors have omitted and which he feels they might better have included. Such an approach by the present reviewer was neatly forestalled with the candid announcement in the introduction:

A characteristic of programming is that, for a thing to be learned, it must be said many times. This implies that a programmed text simply cannot treat as many topics as a standard text of comparable size. This logistic constraint forces the programmer to choose what he considers to be the most salient topics in a subject matter for programming. This leads to inevitable omissions; in such cases, supplementary material must be supplied by the instructor.

The reviewer disagrees strongly with the rationale of this argument. Since the length of an automated teaching program should be determined not by its approximation of the size of a standard textbook, but rather by the nature of the material and the requirements of the satisfactory programming of that material, this explanation for the spottiness of the program does not seem to be a convincing one. However, the reviewer does agree with the authors when they say that supplementary material will have to be supplied to the program by the instructor.

Another approach that a reviewer of a textbook can pursue is to point out statements which are not accurate or precise or with which he is not in agreement. Again we find the TMI staff has anticipated this approach. In their introduction they say:

Another characteristic of programming is that, in order to be clear at a certain point in time, certain qualifications must necessarily be omitted from frames. As a consequence, frames sometimes cannot 'stand alone.' In spite of the fact that such qualifications are added later, such frames often make the purist shudder. The only defense is an appeal to the criterion behavior of the student; if this behavior is satisfactory, the ends appear to justify the means.

The reviewer recognizes the legitimacy of this argument for the occasional 'poor' frame which is corrected later on in the program. Unfortunately, there is a great danger that many poorly worded or imprecise or inaccurate statements will be unjustifiably excused on the basis of this argument. But with regard to the authors' appeal to criterion behavior as the final judge of the effectiveness of a program, with regard to this notion, the reviewer has no reservations. The best way to evaluate an automated teaching program is to determine whether it accomplishes what it is supposed to accomplish. This is also the position taken by Lumsdaine and Glaser (*Teaching machines and programmed learning: a source book*, Washington, D. C.: National Education Association, 1960, p. 566):

It seems clear that standards for the adequacy of a program ought to be conceived primarily in terms of its effectiveness in attaining defined educational objectives,

rather than by specifying the format, sequencing, or other aspects of the means whereby these ends are achieved . . . performance characteristics in terms of the effectiveness with which the student is taught should at present be the basis of any set of standards.

Since there appears to be agreement that the only proper way to evaluate the effectiveness of automated teaching programs is through the use of achievement tests constructed to measure the behavior which the programs purport to teach, then the reviewer must admit that a serious appraisal of *Statistical Inference* cannot be made at the present time and that, consequently, a satisfactory critical review cannot be written. A review written today can contain only subjective comments concerning the program's face validity. The information which is necessary for making a critical evaluation of *Statistical Inference* is not as yet available.

An important question that should be raised at this juncture is: Whose responsibility is it to provide the information about the effectiveness of a particular program, the people who construct and market the program or the people who use it? The members of the staff of TMI apparently feel that it is not their responsibility. At any rate they have not provided the information. Instead, they say that they will gratefully accept whatever information users will send them. They even solicit such information. The reviewer disagrees with TMI and feels that it is the responsibility of the publishers of programs to gather the information and to make it available to the public. No automated teaching program should be published, he thinks, unless information about how the program was tested and evaluated is published along with it. In other words, the purchaser of a programmed textbook should be provided with the equivalent of the now familiar test manual which is published along with every good psychological test. It would be desirable also to have a set of Technical Recommendations for Automated Teaching Devices, as we now have for tests (*Technical recommendations for tests and diagnostic techniques, Psychol. Bull.*, 1954, 51, No. 2, Part 2), in order to guide the publishers in the development

of their manuals. The seriousness of this problem was recognized by Lumsdaine and Glaser (p. 566) who wrote:

In the production of programs a major problem could arise from premature publication and sale of hastily conceived and untested programs. This would pose a serious problem, since flooding the market with such programs could tend to discredit the whole concept of programmed instruction. It would, therefore, appear that a high priority objective is that of working out acceptable quality-control standards for programs. Such standards are more applicable to the development of programs than to specific devices, since it is the programs that carry the burden of subject-matter content and the sequence by which it is taught.

IN line with the reasoning of the above paragraphs, perhaps this review should stop at this point. The reviewer wishes, however, to make his contribution to TMI's experiment in education and accordingly offers the following comments and suggestions.

In Unit 1, several of the stimulus frames ask, incorrectly, for responses in symbolic form instead of in numerical form, e.g., frames S-12, -28, -38, and -39. One of the letters which identifies part of the diagram in frame S-21 is omitted. And finally, either the answers which are given to questions c) and d) on frame S-63 are incorrect, or the questions are ambiguous. In Unit 5, on frame S-28, the words, "the mean of" have been omitted from the first sentence; frame S-59 calls a z-score a statistical test; and frame S-60 cryptically states that 2% of the (normal) curve is at each end. In Unit 6, the frames S-66 to S-70, inclusive, all contain a diagram of two curves which are consistently mislabeled. As a consequence, incorrect answers are given to all the questions raised on these frames. Frame S-43 in Unit 7 requires the student to find a value in a table which is not contained in the table; frame S-52 requests the student to copy a formula which contains a typographical error; and the formula in frame S-69 is printed incorrectly. Finally, the formula in frame S-82 in Unit 9 contains a typographical error.

The reviewer believes that in spite of the presence of the black borders on the frames, with twelve items per page, it is very easy to lose one's place in the middle of the program. He himself had this difficulty. Perhaps it would

help if the complete frame, instead of just the edge, were colored in black or gray. Also, for the same reason, the reviewer feels that the stimulus and the response frames should be of the same size. Furthermore, he believes that requiring the student to turn the page in a backward direction on alternate runs through the program adds unnecessarily to the confusion.

There are many stimulus frames contained in *Statistical Inference* which require more than one response. It seems that this constitutes a violation of the principle of immediate feedback of information (reinforcement), the primary principle upon which this new technique is based. When several responses are required, the student is kept waiting for the information about the correctness of his first response until the succeeding responses have been made. It is suggested, therefore, that either the number of frames requiring multiple-responses be kept to a minimum or, at least, that they be reserved for the end of a unit when the development of the particular terminal behavior with which the unit is concerned is almost complete. Or perhaps the correct responses could be arranged on the response frames so that the page containing the stimulus frame could be pulled back gradually, revealing one response at a time. The development of such a system might be worth considering for another reason. Not only would it enable the immediate reinforcement of all responses, even when several responses are required, but it would also make possible the substitution of oral responses for the more time-consuming, and therefore more tedious, written responses which are now required in many instances solely for the purpose of temporary storage. The student, for example, could be relieved of the unnecessary task of writing out his choice of such alternatives as: *is/is not* or *proves/disproves*.

In conclusion, the reviewer reminds the reader that the publishers of TMI have offered *Statistical Inference*, not as a cure for all that ails modern education, but as a new technique which needs testing. They admit that their program will have to be revised. They do not promise that everyone will find it use-

ful. They imply that they themselves have found it to be effective, but they insist that the purchaser of the program try it for himself. They do not promise that the student will enjoy working on the program, that he will learn from it, nor that he will remember what he learns. Nor do they suggest that the program be used in any one specific manner. They confess that they do not know what the optimal procedure for its use is. When one picks up a copy of *Statistical Inference* and prepares to use it, he should perhaps be reminded of a sign which the motorist often sees when he is touring new country: *Road under construction. Travel at your own risk.*

Mental Disorder Viewed en Masse

Benjamin Passamanick (Ed.)

Epidemiology of Mental Disorder.

(A Symposium organized by the American Psychiatric Association to commemorate the centennial of the birth of Emil Kraepelin, co-sponsored by the American Public Health Association, New York, 27-28 Dec. 1956.) Washington, D. C.: American Association for the Advancement of Science, 1959 (Publication No. 60). Pp. x + 295. \$6.50 (\$5.75 to AAAS members).

Reviewed by BERT KAPLAN

The reviewer, Dr. Kaplan, is Associate Professor of Psychology at the University of Kansas, where he has been teaching since 1953. He is a Harvard PhD with special interests in social psychology and anthropology and is editor of Microcard Publications of Primary Records in Culture and Personality. He reviewed Miller and Swanson on The Changing American Parent (Wiley, 1958; CP, Oct. 1959, 4, 318f.).

EPIDEMIOLOGY has been described as the study of the "mass" aspects of disease. The present volume, a report on the proceedings of a 1956 symposium,

jointly sponsored by the American Psychiatric Association and the American Public Health Association, is ample evidence that a new field of research has emerged which breaks with the traditional concern of psychiatry with the etiology and treatment of the single case, and instead treats the mental disorders as mass phenomena whose form and rate of occurrence may be influenced by factors which are quite different from those that are involved in a single case. The social settings and ecological variables have the greatest claim to the attention among the researches in this volume, but genetic, racial, age, and even seasonal influences are also considered.

The symposium is a lively dialogue between eleven teams of research workers, who represent most of the major epidemiological projects in progress in 1956, and eleven authorities who with considerable wisdom assess, criticize, and interpret the research reports. These reports discuss surveys of the prevalence of psychoneurosis and psychosis in a variety of populations, the association of the conditions described with socioeconomic status, housing, and other variables, factors influencing prognosis, treatment, and emotional complications during pregnancy, and such miscellaneous matters as seasonal variations in hospitalization for old-age psychosis, the distribution of intelligence in an infant population, and the nature of personality changes occurring during the second decade of life.

The studies taken as a group provide few conclusions of any great significance. Many of the findings, such as the great seasonal variation in hospitalization rates, appear at first sight to be quite dramatic but on second thought prove to be less interesting end-products of psychiatric practices or institutional arrangements. The epidemiologist's problem is well illustrated by Milton Terriss' comment following one of the papers. He noted that during World War II psychiatric rejection rates varied from $\frac{1}{3}$ in one station to 51% in another. In Pittsburgh, 24% of the men examined were rejected as compared with 7% in Philadelphia; in Seattle the rejection rate was 22% but in San Francisco it was only 5%. It is almost cer-

tain that the variation in rates was a result of different policies and practices at the various induction stations rather than differences in the psychiatric characteristics of the populations of these cities.

In general it appears that the researchers are as aware of these difficulties as are their critics. The main difference is that the researchers are more willing to minimize the defects and to find reasons for going ahead. One can admire and respect their efforts and wonder whether their work is not more important than the sophisticated scepticism of their critics.

This reviewer feels most keenly Daniel Wilner's plaintive comment in reporting on his and Rosabelle Walkley's attempt to discover whether housing environment per se influences mental health. Wilner says that after two and a half years of intense study that utilized substantial resources in both scientific sophistication and man-hours, they are still "without major findings." To his addendum that there are "none in sight for perhaps another year" we must pessimistically add "and perhaps not even then." These writers are more than normally candid in asking, "Having gone this far, can the study really be done at all?"

What seems apparent—to this reviewer at least—is that the methods being brought to bear on the really difficult and complex questions that are typical of epidemiological research are still inadequate, and that it is not enough to borrow the simple techniques and research designs that have been developed in other areas. The questionnaire, the personality inventory, the brief interview, the hospital admission rate, the index of class position, and many other procedures are easily available but unfortunately often yield ambiguous and uninterpretable results. The moral of the present batch of projects is that this field of research cannot rest on an already established methodology but must move forward and develop its own if it is to realize its enormous potential. The present researches are in the main reasonable and thought-provoking, yet they represent a stage of scientific adolescence or even scientific infancy rather than maturity.

ON THE OTHER HAND



DOLLARD, AULD, OBSERVATION, AND INFERENCE

In reviewing Dollard and Auld's *Scoring Human Motives*, Timothy Leary (*CP*, Oct. 1960, 5, 337f.) does not report the authors' clearly stated purposes in writing their book, nor does he discuss how well he thinks they have accomplished their purposes. Instead he abuses them for an alleged inadequacy in dealing with issues with which Leary has become concerned since he turned from psychotherapy to philosophy but to which Dollard and Auld do not and need not address themselves. The sneer and the sarcasm are so unjustified.

Leary bases his review on a discussion of what he calls epistemology—the determination of Fact in an ultimate objective way, a way that philosophers talk about: what is *really* going on inside the patient. His analogue is the perception of a mirage, which Leary happens to know is *really* not there. But this is utterly irrelevant to the purposes of the book under review—as it is for most scientific purposes. No scientist hopes to stand in Leary's exalted position to know what is *really* there. Dollard and Auld understand clearly that all we have in studying human behavior—in psychotherapy or anywhere else—are facts, not Facts. We have observations and we have inferences made by observers. These happen to be enough for science, even though not enough for Leary. Science undertakes to develop interpretations and theories which may be sensibly related to observations.

This relation between observations and theoretical interpretations is, however, an especially difficult problem in psychotherapy where both observations and interpretations abound, as Leary has catalogued, but where the links between are so seldom stated. Dollard and Auld address themselves precisely to determining some specific rules for linking observations and inferences. They want to discover and perhaps to modify or to ease what goes on "in the mind of the interpretive observer," as Leary puts it. The interpretive observer who puts the label *anxiety* on a bit of behavior is getting from his observation to his interpretation by some process. Dollard and Auld want to make it explicit. Whether

either observation or inference is related to Ultimate Fact is beside the point. The question is whether Dollard and Auld are able to develop rules that allow one to move back and forth from observable facts to theoretical interpretation in a way that provides for a relatively powerful theoretical control over a relatively wide range of observable facts. There may be a real question as to how adequately they have accomplished this task, but a review of their book ought to discuss this issue, not abuse them over other matters.

It is an incredible misreading to say that Dollard and Auld miss this distinction. Dollard and Auld explicitly call attention to this gap between observation and inference and just as explicitly declare that it is their intention to find lawful procedures for closing the gap.

Perhaps Leary has the idea that observation and inference are inevitably separated for all time, as quite independent avenues to ultimate facts. He chooses to focus his scorn on Dollard and Auld's assumption that "sentences always have defining emotional and other reactions attached to them." Either one must recognize this as the very point of the book—as Leary has not—or else despair over any science or any practice of psychotherapy—as Leary apparently has.

Leary is welcome to move from science to philosophy, as *CP* reports that he has; but he has no right to insist that others follow him in this act of despair—not, at least, until he has examined their work to see whether it happens to deal, more successfully than he apparently feels his own has, with some of the difficult scientific problems.

JAMES E. DITTES
Yale University

FOR WHOM DOES THE TRUMPET SOUND?

In its first issue, *CP*'s purpose is described as "picturing the current scene by telling its readers what its books contain," although "its reviewers are asked not merely to abstract books but to criticize them, both negatively and positively, to put them in perspective, and to suggest their significance in modern psychology"

(*CP*, Jan. 1956, 1, 13). I interpret this to mean that *CP*'s reviews are to be restricted to issues directly involved in or raised by its books, a policy which seems to have been followed quite closely by most of its reviewers. Therefore, the fact that one recent reviewer has on two separate occasions introduced original material irrelevant to the content of the book being reviewed strikes me as definitely out of place and worth calling to *CP*'s attention.

I am referring to S. S. Stevens' reviews of Thurstone's *The Measurement of Values* (*CP*, Dec. 1959, 4, 388f.), and of Hogben's *Statistical Theory* (*CP*, Sept. 1960, 5, 273-275), and particularly to the reviewer's usurpation of *CP*'s pages to present original points of view that are not discussed in either book nor relevant to either. This feature is most obvious in the case of Thurstone, whose accumulated life work receives but half a page of cursory examination, followed by nearly equal space devoted to the reviewer's ruminations over how Thurstone might react now that Stevens believes he has shown Thurstone's approach to be dead wrong. Any psychologist of Thurstone's stature is clearly deserving of far better treatment from *CP*, especially if this should turn out to be the last chance for his work to be discussed within its pages. While Hogben gets far more comprehensive and sympathetic treatment, this is attributable to the unusually large space allotment; the reviewer's discussion of another favorite topic, this time the role of type of measuring scale in the choice of statistics, approaches in length many of *CP*'s complete reviews. Yet in neither case are these issues even mentioned in the books under review, besides which they have previously been expounded at length elsewhere by Stevens, and do not represent anything new or different.

The fact that I disagree with Stevens' points of view on these issues is really irrelevant to my objection. The essential question remains: should *CP*'s reviewers be required to restrict themselves to issues related to the content of the book they are reviewing, or not? I submit that they should, and that any other course is unfair to the author and to *CP*'s readers, as well as demeaning to *CP*'s otherwise deservedly high reputation.

WILLIAM F. BATTIG
University of Virginia

A BOON FOR BEING FIRST-BORN?

In his review of Schachter (*CP*, Oct. 1960, 5, 328f.) Joseph Veroff states that a great discovery is the importance of birth order and cites as one finding, "first-borns

or only children become more committed to psychotherapy."

I happen to be associated in two unrelated research projects with persons in psychotherapy, and in neither do first-borns appear at the chance level of expectancy.

In one study (in which nine psychiatrists are associates) there are 106 homosexual and 100 heterosexual males in private psychoanalytic treatment. There are 11 only-children among the homosexuals, and 22 among the heterosexuals. For those patients having siblings the groups are similar enough to be combined. Of the 73 having one sib, only 28 are first-born; of the 42 having 2 sibs, only 10 are first-born; of the 35 having 3 sibs, only 1 is first-born; of the 23 having 5 or more sibs, only 2 are first-born. The patients are underrepresented as first-born at every family size.

The other study, which is in collaboration with B. F. Riess and Clifford Sager, is a follow-up of a sample of 201 cases from the Postgraduate Center, who had had a year or more of treatment. This should be a lower socioeconomic group than the patients in the study of male homosexuality, and it includes both women and men. Only-children constitute about 15% of the population. For those with siblings, there are about 10% fewer than the expected first-borns.

Maybe first-borns are less committed to psychotherapy! At any rate there certainly are within different family constellations dynamic factors in which birth order is an important consideration.

RALPH H. GUNDLACH
New York City

HOW TO WRITE THE PUBLISHER

It would be helpful to those of us in the provinces who read *CP* with an eye to purchasing books, if *CP* would print the addresses of the publishers. Some of them are well known and readily locatable, but others, like the Group for the Advancement of Psychiatry and the New York Academy of Sciences, are not. An additional line of type with each title would serve.

DEAN A. ALLEN
Bowdoin College

CP will try to oblige, beginning right now, though the change will emerge only slowly. Every improvement is more work for CP but the idea is good. CP will not, however, add the addresses in the list of BOOKS RECEIVED, just in the reviews.



Recent Slavic Books in Psychology

Compiled by JOSEF BROŽEK
Lehigh University

Last month CP published Dr. Brožek's list of recent Russian books in psychology. The present list is for Slavic countries other than Russia.

Bulgarian

PIRYOV, G. D. *Kum psikhologiyata na naglednostta i suznatelnostta pri obuchenieto* (Psychological basis of objective teaching and consciousness in the process of education; English and Russian summaries). Sofia: Bulgarian Academy of Sciences, 1958. Pp. 166.

PIRYOV, G. D. *Detska psikhologiya i defektologiya* (Child psychology and the study of handicapped children). 1959. Pp. 556.

Czech and Slovak

AUTORSKÝ KOLEKTIV (Collective of Authors). *Problémy dětských úrazů* (Problems of children accidents). Brno: Czechoslovak Society for the Dissemination of Political and Scientific Knowledge, 1958. Pp. 97.

ČILUP, OTOKAR. *K psychologickým základům výchovy a vyučování* (Psychological foundations of education and teaching). Prague: State Pedagogical Publishing House, 1959. Pp. 339.

GUENSBERGER, ERNEST (Ed.). *Hraničné problémy psychiatrie* (Border problems of psychiatry; summary in English). Bratislava: Slovak Academy of Sciences, 1957. Pp. 283.

GUTMANN, ERNEST (Ed.). *Sborník prací konference o nervovém metabolismu a aktivním transportu iontů* (Symposium on nervous regulation of metabolism and active transport of ions). Prague: Czechoslovak Academy of Sciences, 1958. Pp. 195.

HRBEK, JAROMÍR (Ed.). *Vyšší nervová činnost člověka* (The higher nervous activity of man: A collection of studies; summaries in Russian, English, and German). Prague: State Pedagogical Publishing House, 1958. Pp. 304.

KOVÁLIKOVÁ, VERONIKA (Ed.). *Využití psychologie v socialistické společenské praxi* (Utilization of psychology in the socialist social practice: Proceedings of the first conference of Czechoslovak psy-

chologists). Bratislava: Slovak Academy of Sciences, 1959. Pp. 403.

MICHALOVÁ, C., J. ČELEDÁ, J. BUCHNÍČEK, AND J. LINHART. *Filosofie a přírodní vědy* (Philosophy and natural sciences; contains a section by J. Linhart on "Reflex character of psychic activity," pp. 377 to 480). Prague: State Publishing House of Political Literature, 1959. Pp. 486.

NATIONAL MEDICAL LIBRARY. *The annual of Czechoslovak medical literature*. Prague: State Health Publishing House, 1959. Pp. XXI + 509.

PODOBA, JULIÁN (Ed.). *Problémy endokrinologické praxe z hlediska výskumu* (Practical problems in endocrinological research; abstracts in Russian, German, and English). Bratislava: Slovak Academy of Sciences, 1958. Pp. 187.

RUTTKAY-NEDECKÝ, IVAN, AND VLADISLAV ZIGMUND. *Psychobiografický rozbor životopisu* (Psychobiographic analysis of curricula vitae; Russian, German, and English summaries). Bratislava: Slovak Academy of Sciences, 1957. Pp. 124.

UHER, BOHUMIL (Ed.). *Studie z pedagogiky a psychologie* (Studies in education and psychology; from the Higher Pedagogical School in Brno). Vol. 4. Prague: State Pedagogical Publishing House, 1958. Pp. 253.

Polish

BALEY, STEFAN. *Psychologia wychowawcza w zarysie* (An outline of educational psychology; 4th ed.). Warsaw: State Scientific Publishing House, 1958. Pp. 415.

BALEY, STEFAN. *Wprowadzenie do psychologii społecznej* (Introduction into social psychology). Warsaw: State Scientific Publishing House, 1959. Pp. 227.

FRANUS, EDWARD. *Rozwój reakcji gniewu małych dzieci* (Development of anger reactions in small children; English summary). Cracow: Jagiellonian University, 1959. Pp. 135.

HORNOWSKI, BOLESŁAW. *Analiza psychologiczna testu percepcyjnego* (Psychological analysis of a perceptual test, J. C. Raven's "progressive matrices;" English & Russian summaries). Warsaw: State Scientific Publ. House, 1959. Pp. 213.

- HORNOWSKI, BOLESŁAW. *Psychologiczne podstawy kształtowania poglądu na świat dzieci i młodzieży* (Psychological bases of the formation of the outlook on the world in children and youth; English summary). Poznań: State Scientific Publishing House, 1959. Pp. 173.
- LEWICKI, ANDRZEJ. *Procesy poznawcze i orientacja w otoczeniu* (The cognitive processes and orientation in the environment; English summary). Warsaw: State Scientific Publishing House, 1960. Pp. 242.
- PRZETACZNIKOWA, MARIA. *Odzwierciedlanie cech przedmiotów i zjawisk w mowie dzieci w wieku przedszkolnym* (The reflection of the characteristics of objects and phenomena in the language of preschool children; French summary). Cracow: Jagiellonian University, 1959. Pp. 217.
- SŁONIEWSKA, HELENA. *Psychologiczna analiza zainteresowania* (Psychological analysis of interests; English summary). Poznań: State Scientific Publishing House, 1959. Pp. 110.
- SZEWCUK, WŁODZIMIERZ. *Analiza psychologiczna zawodu kierowcy suwnicy* (Psychological analysis of the work of crane operators). Cracow: State Scientific Publishing House, 1959. Pp. 65.
- SZEWCUK, WŁODZIMIERZ. *Psychologia człowieka dorosłego: wybrane zagadnienia* (Psychology of the adult man: Selected problems). Warsaw: Wiedza Powszechna [Universal Science], 1959. Pp. 131.
- WŁODARSKI, ZIEMOWIT. *Zaburzenia równowagi procesów nerwowych u dzieci* (Disturbances of the equilibrium of the nervous process in children). Warsaw: State Scientific Publishing House, 1960. Pp. 155.
- Serbo-Croatian**
- BUJAS, ZORAN, AND BORIS PETZ. *Osnove psihofiziologije rada* (Principles of the psychophysiology of work: Introduction into industrial psychology). Zagreb: Institute for Industrial Hygiene, Yugoslav Academy of Science and Arts, 1959. Pp. 420.
- (Financial aid provided by the Institute of Research, Lehigh University, in obtaining the Slavic publications is gratefully acknowledged by the compiler.)
- BOOKS RECEIVED**
- BRAZIER, MARY A. B. (Ed.). *The central nervous system and behavior*. (Transactions of the Third Conference, 21-24 Feb. 1960.) New York: Josiah Macy, Jr. Foundation, 1960. Pp. 475. \$7.50.
- BRITT, S. H. *The spenders*. New York: McGraw-Hill, 1960. Pp. xiv + 293. \$4.95.
- COHN, ROBERT. *The person symbol in clinical medicine: a correlation of picture drawings with structural lesions of the brain*. Springfield, Ill.: Charles C Thomas, 1960. Pp. xiv + 196. \$10.00.
- COVILLE, W. J., T. W. COSTELLO, & F. L. ROUKE. *Abnormal psychology; mental illness: types, causes, and treatment*. (College Outline Series, No. 94.) New York: Barnes & Noble, 1960. Pp. xvi + 298. \$1.75.
- DECKER, FRIEDA. *Progressive lessons for language retraining*. Book 1: *The Days at home*. Book 2: *Mr. and Mrs. Day go shopping*. Book 3: *The Days take a trip*. Book 4: *The Days buy a house*. New York: Harper & Brothers, 1960. Pp. viii + 71; viii + 71; viii + 71; viii + 71. \$1.50 each.
- DEUTSCH, MARTIN. *Minority group and class status as related to social and personality factors in scholastic achievement*. (Monograph No. 2.) Ithaca, N. Y.: Society for Applied Anthropology, New York State School of Industrial and Labor Relations, Cornell University, 1960. Pp. 32. \$1.50.
- DISHER, DOROTHY R. (Ed.). *Workbook for students of adolescent adjustment*. Garden City, N. Y.: Author, 1960. Pp. vi + 201. \$3.50.
- EISSLER, RUTH S., ANNA FREUD, HEINZ HARTMANN, & MARIANNE KRIS (Eds.). *The psychoanalytic study of the child*. Vol. XV. New York: International Universities Press, 1960. Pp. 481. \$8.50.
- FREUD, E. L. (Ed.). *Letters of Sigmund Freud*. (Trans. by Tania & James Stern.) New York: Basic Books, 1960. Pp. viii + 470. \$7.50.
- GEERTZ, CLIFFORD. *The religion of Java*. Glencoe, Ill.: Free Press, 1960. Pp. xvi + 392. \$7.50.
- HAMMOND, K. R., & FRED KERN, JR., with W. J. CROW, J. H. GITHENS, BYRON GROESBECK, J. W. GYR, & L. H. SAUNDERS. *Teaching comprehensive medical care: a psychological study of a change in medical education*. Cambridge, Mass.: Harvard University Press, for the Commonwealth Fund, 1959. Pp. xxii + 642. \$10.00.
- INTERNATIONAL BUREAU OF EDUCATION. *Organization of special education for mentally deficient children*. (XXIIIrd International Conference on Public Education, Geneva, 1960.) Paris: UNESCO; Geneva: International Bureau of Education, 1960 (distributed by International Documents Service, Columbia University Press, New York). Pp. 272. \$3.00.
- JACO, E. D. *The social epidemiology of mental disorders: a psychiatric survey of Texas*. New York: Russell Sage Foundation, 1960. Pp. 228. \$3.50.
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